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SUPPLEMENTS TO THE BULLETIN OF THE HISTORY OF MEDICINE

Editor: HENRY E. SIGERIST
Associate Editor: GENEVIEVE MILLER

No. 9

NOAH WEBSTER:

LETTERS ON YELLOW FEVER ADDRESSED TO DR. WILLIAM CURRIE

WITH AN INTRODUCTORY ESSAY BY

BENJAMIN SPECTOR

BALTIMORE
THE JOHNS HOPKINS PRESS
1947

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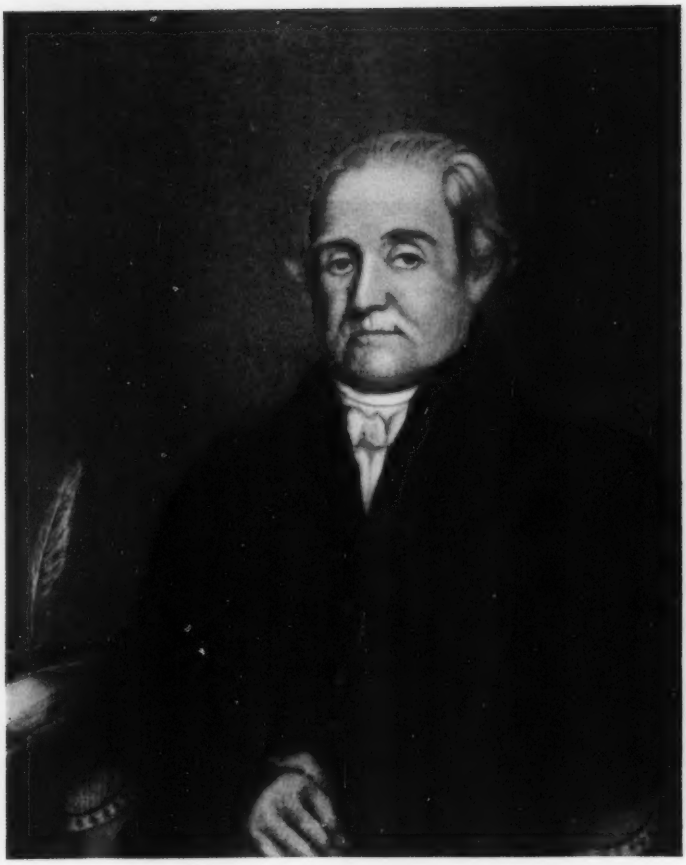

NOAH WEBSTER

NOAH WEBSTER

His Contribution to American Medical Thought and Progress

Although Noah Webster has achieved immortality as a lexicographer, no student of the history of medicine in the United States would think of denying a notable place to him in the annals of medical literature. The writer believes that Webster can best be understood if his writings are examined in the light of the thought so well expressed by Hippocrates in the book of the *Epidemiae*: "Declare the past, diagnose the present, foretell the future . . . The art (of medicine) has three factors, the disease, the patient, the physician." [1]

A critical reading of "A Collection of Papers on the Subject of Bilious Fevers Prevalent in the United States for a Few Years Past"; [2] "Letters to Dr. William Currie"; [3] and "A Brief History of Epidemic and Pestilential Diseases; with the Principal Phenomena of the Physical World, which Precede and Accompany them, and Observations Deduced from the Facts Stated" [4] will furnish ample evidence that Webster thought and wrote in the Hippocratic spirit noted above. That these writings constitute fundamental building blocks upon which the modern edifice of epidemiology was subsequently raised has already been commented upon favorably by such eminent scholars as Billings,[5] Warthin,[6] Winslow,[7] Creighton,[8] and Warfel.[9]

One who reads history aright soon finds that he who preserves is as important as he who serves. Webster, be it said, did both. He served American medicine by his diligent collection of facts pertaining to yellow fever, obtained through communications with physicians and by personal, careful, observations. He offered a useful distinction between contagion and infection, particularly at a time when there was no adequate knowledge of the germ or of human carriers of disease to support the concept of infection. In opposition to the local miasmatic theory of disease, he became a fervent protagonist of the principle of the Epidemic Constitution.

His *Brief History of Epidemic and Pestilential Diseases* preserves for us a chronology of this important subject presented in a manner better than any writings before it.[7] In pointing the way to a new level of critical thinking, by urging the necessity of continued investigation into the cause of diseases, and repeated insistence on truth based upon facts, Webster made a place for himself not only as a wrecker of theories but as a builder of true scientific attitudes.

Noah Webster was born in West Hartford, Connecticut, on October 16, 1758 and died on May 28, 1843. Within these eighty-five years, the advancement of science ranged from Franklin's demonstration of bringing electricity down from the clouds to the discovery of the contagiousness of puerperal fever. During this period, scientific observations and experiments were made which, in later years, unfolded into our present state of scientific medicine.

Webster received his early training from the local minister and schoolmaster. In 1774, he was admitted to Yale College from which he graduated, four years later, with the degree of B. A. Subsequently, Yale conferred upon him the degree of M. A. Webster had settled upon a legal career, and after reading law with various jurists, he was admitted, in 1781, to the bar at Hartford. For several years, he practiced law in Hartford then, in 1793, he went to New York to become editor of a new daily newspaper, *The Minerva* and of a semi-weekly paper, the *Herald*. In 1783, he published his immortal *American Spelling Book*, which ran through more than 60,000,000 copies. The passage of the first legislative provision for an American copyright was due to his initiative, patriotism and leadership. This activity brought him into politics and he became an ardent Federalist. He lectured on the mother tongue in Baltimore and later in Philadelphia, where he came to know Benjamin Franklin. His was not a parochial mind; he wrote various treatises in the fields of politics, economics and physical science. As Secretary of the Connecticut Academy of Arts and Sciences, he drew up a memorandum to petition Congress to obtain vital statistics on the health and longevity, among other matters, of the citizens of the United States.[7] His observations in climatology gave useful information to the census and weather bureaus. His scientific publications appeared in the *Medical Repository*[10] and in

the *Memoirs of the Connecticut Academy of Arts and Sciences.*[11] These many-sided labors were an excellent preparation for his scholarly achievement which came out, in 1828, under the title *An American Dictionary of the English Language.* This work was an outstanding contribution to American culture. It was during his residence in Amherst, Massachusetts, that he called the meeting which laid the foundation of Amherst College. In 1822, he returned to New Haven and in the following year was honored with the degree of LL. D. by Yale College.[12]

Webster was a man of deep religious faith and great pertinacity. His versatile and alert mind is a revealing example of a layman whose interests and efforts contributed to the medical thought of the day, particularly at a time when theory, empiricism and authority were dominant forces in medicine. He may have been influenced by the letter which Benjamin Franklin wrote to the Rev. John Lathrop —an eminent clergyman in Boston—under date of May 31, 1788. Franklin wrote: [16] " The present progress is rapid . . . If the art of physic shall be improved in proportion with other arts, we may then be able to avoid diseases and live as long as the Patriarchs in Genesis; to which I suppose we should make little objection." It was not unusual, however, in the early days of our country's history to find laymen taking a vital interest in the health of the people. Cotton Mather, Benjamin Franklin, Mathew Carey,[20] and in later times, Lemuel Shattuck—although not physicians—participated in matters concerning the general welfare of the people. Precedents for this sort of activity were already established on the Continent: in England by the publications of Lady Montague, John Howard, and later of Edwin Chadwick, Jeremy Bentham; in France, by the work of Pasteur.

Only if we are aware of the multiform background of the time in which Webster lived, can we form a reasonable estimate of the sincerity, boldness and impelling faith which urged him to enter the field of medical thought. The latter part of the eighteenth and the early part of the nineteenth centuries were characterized, to a considerable extent, by efforts of men of great intellect to promote and diffuse useful knowledge. The American Philosophical Society was established in 1768—ten years after the birth of Webster. Its

first president was Benjamin Franklin, its second David Rittenhouse, and its third Thomas Jefferson. The example of these distinguished men—with all of whom Webster corresponded—fired his interest to extend such knowledge as would increase the welfare of the young growing nation. Those were times when it was held that no knowledge which is truly knowledge is useless if it contributed to the comfort, security and happiness of the people. Webster constantly repeats this motivation in his writings, and for this reason it may be assumed that his guiding principle was the Ciceronian idea embraced in the words " Salus populi suprema lex esto."

Technologically, the era we are considering saw the development of the spinning jenny (1764), the power loom (1785), the cotton gin (1793), practical steamboat navigation (1807), and the first use of railroads (1830). These developments emphasized the importance of exact measurements in the advance of science. Equally important was the awakened interest in the subject of quantitative analysis which resulted in the discovery of carbonic acid gas by Black (1754); of hydrogen by Cavendish (1766); of oxygen by Priestley (1774) and of the appearance in 1789 of Lavoisier's epoch-making *Traité élémentaire de chimie*. These facts supplied the necessary props for certain theories respecting the causation of disease.

In the political field, there quickly comes to mind the Stamp Act of 1765, the American Revolution of 1776, the Struggle for the Constitution of the States (1787-89) and the French Revolution of 1789-99. These events were important in shaping Webster's thinking, particularly with regard to the happiness of the people, and it was especially through this channel that he was carried into the areas of conflict of the medical theories of disease.

With respect to the social organization of the times, it is interesting to point out that at the commencement of the Revolutionary War, thirty-seven newspapers were being published in the American Colonies.[13, 19] While most of the newspapers followed the policy of supporting or attacking the Administration, Webster strongly felt that he ought to go beyond political considerations and inform the public concerning such facts and principles as were pertinent to the intelligent control of disease. This aim, as we shall later see, he carried out admirably as editor of the *New York Commerical Advertiser*.[14]

In the field of higher education, the following universities had been established when Webster was six years of age: Harvard (1636); William and Mary (1693); Yale (1701); Princeton (1746); Pennsylvania (1753); Columbia (Kings) (1754); and Brown (1764). And yet, in 1800, in a volume of letters to Dr. Priestley, Webster writes that "our colleges are disgracefully destitute of books and philosophical apparatus."

At the commencement of the Revolutionary War this country had one medical book by an American author, three reprints and about twenty pamphlets. The first American medical journal was a quarterly, the *Medical Repository*, established and published in New York in 1797. Public and private libraries were growing in numbers.

The population between 1700 and 1760 increased almost sevenfold, from 200,000 to about 1,600,000. This pointed up the increasing need for public health measures. Although the first local board of health in America was organized in Baltimore in 1793, and the second in Philadelphia in 1794, as a consequence of a disastrous yellow fever epidemic, the first *state* law authorizing the establishment of local boards of health was passed in Massachusetts in 1797, only two years before Webster published his great *History of Epidemic Diseases*.[15]

Economically, the country was young and struggling to finance itself. The question of whether yellow fever arose from local miasmatic conditions and, therefore, should be controlled by sanitation was of vital concern to the country. If this idea prevailed, the measures necessary for carrying out quarantine would interfere with the commercial development of the fast growing cities. The introduction of the "sanitary idea" involved the whole question of economics. Webster, we shall see, took an active part in the controversy over the merits of quarantine. In a letter to Timothy Pickering, dated October 20, 1799, he wrote: "Although I would not relax, but improve the regulations for preventing the introduction of infectious diseases into our seaports, yet I have satisfied myself that nine out of ten and more, of our pestilential autumnal fevers are the produce of our own atmosphere and are not to be reached by Health Laws . . . I will challenge all the faculty in America to assign causes, consisting in *visible* and tangible substances and as

fast as they are assigned, I will disprove them by incontrovertible facts. In short, I can demonstrate that the principle which gives to fevers the pestilential quality consists in the *insensible* properties of the atmosphere." [7]

The tenor of thought in medicine during these times depended upon the particular hypothesis which was held by this or that group of physicians. This was an era of systems of medical theories with those of Cullen [16] and Brown [17] holding the stage. To Webster's penetrating mind, the most fertile hypothesis for an understanding of the spread of disease was the Epidemic Constitution.

The prevailing climate of medical opinion was brisk, invigorated by the energies of such individuals as Benjamin Rush, Samuel Bard, and William Shippen, Jr. To be sure, the base of activity stemmed from the establishment of the first medical school at the College of Philadelphia in 1765, which followed shortly after the appearance of Dr. John Morgan's book entitled *A Discourse upon the Institution of Medical Schools in America.*

A Brief History of Epidemic and Pestilential Diseases.[4]

The writing of this book grew out of the great need for both physicians and the general public to learn about the dreadful disease called yellow fever. The appearance of the disease was of more consequence to the United States than even the political state of the country, for war, at worst, was a temporary calamity; but the autumnal, malignant fever, if generated in our climate, was a calamity that bid fair to appear every year in perpetuum. With this comprehensive survey, the first of its kind in this country, Webster broke new ground for American scholars in the presentation of our knowledge of epidemiological data. The chief merits of the volumes under consideration are to be found not in the manner of arrangement and treatment but in the abundance of material gathered together. The author achieves a breadth of view worthy of the best physicians of his day. It can be safely asserted that much may be learned today by a painstaking reading of its contents.

This book, written when Webster was not quite forty years of age, is a synthesis of historic thought with a thesis which is well

expressed in the concluding chapter of the book addressed to the learned societies, in America, Europe and Asia, as follows:

> The preceding history of Epidemic diseases was undertaken solely from a desire of investigating the truth, respecting the origin, and phenomena of these terrible scourges of the human race. When the pestilence appeared in the United States in 1791 and 1793, I had not a suspicion that the popular doctrines respecting contagion are not well founded. The frequent recurrence of the disease in subsequent years, in opposition to all the best efforts of health officers, in executing rigid laws of quarantine, had in 1795, shaken my confidence in these doctrines. My investigations in that and the next year, convinced me that the pestilential fever which has visited so many parts of America, is generated in the country; but still I had not the least suspicion of a connection between epidemic diseases. The investigations of the year past, have resulted in unfolding principles and facts to me altogether *new* and *surprising*; they cannot therefore be ascribed to a wish to establish a preconceived theory.

Here is a striking example of a lay American citizen who, without a preconceived theory to maintain and with little more than an intense desire to establish truth based on facts, entered the arena of medical discussion by way of the investigation of the cause of epidemic diseases.

Why did Webster undertake this work? For one thing, he was deeply interested in the general welfare of the people in terms of hard, practical facts, not out of a feeling of myopic patriotism but from a highminded idealism. Another reason lay in his observation that too many authors dealt with what to him were disgusting details of political intrigue and war and omitted or only briefly mentioned problems concerning the health of the nation. A third reason may be found in the *libido sciendi*, which was deep-seated in him.

It was in the year 1795, after observing the *progressiveness* of epidemic diseases, that Webster came to the opinion that the common theory of infection was ill-founded. This should cause no eyebrow-raising when it is recalled that, even eighty years later, a distinguished professor of medicine in New York (Dr. Alfred L. Loomis) went on record that bacteria found in connection with the development of disease were the product and not the cause of disease processes. Webster believed that the effectiveness of legislative and police regulations depended upon settling the controversy over

the imported or domestic origin of this disease, a point which was then agitating the minds and stretching the *pia mater* of physicians.

In the Hippocratic spirit of acquiring information for the purpose of foreknowing and foretelling disease, he addressed a circular letter (October 31, 1795) to the physicians of Philadelphia, New York, Baltimore, New Haven and, in general throughout the United States, requesting all facts which could throw light on the question of the foreign or domestic origin of the yellow fever.

The full weight of the evidence collected by Webster to show the chain of inductive reasoning which led to his support of the hypothesis of the epidemic constitution cannot here be given, yet the following points are worth noting. Inasmuch as physicians were unable to account for the pestilence on the principle of extraordinary seasons and certainly disdained to admit the influence of the planets to be the cause, they turned to invisible animalculae and to infection concealed in bales of goods or old clothes as the probable causes for diseases which scourged mankind. However, the whole doctrine of the propagation of the plague from country to country by infection cannot be reconciled with the principles which modern observations have unfolded; on the contrary, there is definite evidence of the existence of a pestilential principle in the elements. To demonstrate this latter fact one need only to turn to the bills of mortality in London, Amsterdam, Vienna, Dresden, Boston, and Philadelphia to observe everywhere the effects of a general unhealthy state of the air. The operation of this principle of the epidemic constitution is world-wide; it afflicts mankind in all climates. The epidemic pestilences, though more or less infectious, begin and end at the command of the elements, under the cooperating influence of seasons and *local* causes.

The following important observations appear to overthrow the preconceived idea of the origin of pestilence, in temperate latitudes, from fomes conveyed to those climates from southern regions. In the first place, if, on the appearance of pestilence in a particular place, all other diseases cease, or take some of its predominant symptoms, that pestilence is *generated in that place*, and is dependent on the state of the elements. This idea of the cessation of diseases and the replacement of other types by the prevailing type of pesti-

lence is a typical Hippocratic-Sydenham view; that is to say, if yellow fever prevails then malaria and respiratory diseases cease. Another criterion is the prevalence of certain *diseases before and after* the pestilence. For example, if pestilence is immediately preceded by measles, affections of the throat, inflammatory or typhus fever, especially by catarrh, that pestilence is an epidemic, *produced on the spot*. A third criterion is the fact that although epidemic pestilences are more or less infectious, such infection extends to the distance of a few feet only, and is capable of dissipation in a free air, so as to reduce the danger of attending the sick almost to nothing. Diseases, then, dependent on infection only are propagated by contact or near approach only, and do not effect the character of other diseases current in the place. The consequence is that epidemic pestilence is not under human control although diseases propagated by mere infection may be arrested and subdued. This opinion was quite plausible one hundred years ago when it is recalled that nothing was known about the germ theory or about the concept of change in virulence of organisms. Understanding this viewpoint, it is clear why Webster argued that the only means of avoiding or mitigating epidemic pestilence are first, to withdraw the aid of local causes; second, to fit the body by modes of living to resist its causes; and third, to remove from the place where it exists, if the other two fail. The great desideratum is not so much to guard against infection as against debility through excessive or reduced excitability. It is to Webster's credit that he came to the opinion of the necessity for the preservation of the tone of the system from observation and conversation with physicians, *before* he had read Brown's " Elements of Medicine."

We return to the common doctrine of contagion which then was accepted as the cause for pestilence and which Webster considered utterly insufficient and unphilosophical. His argument was substantially this: If we trace the yellow fever, for example, to the West Indies, and the plague to Egypt or Constantinople, we are not an inch nearer to the source; for these diseases are not always to be found in those countries; and the people there are as much puzzled to find the source of them, as the people of Great Britain or America. Indeed nothing is more common than for the yellow fever to be

imported into the West Indies in vessels from the United States. When vessels from northern latitudes have long passages, it often happens that seamen are seized with the disease, before they arrive at the islands; and the West-Indians may often allege the disease to be *imported in such vessels, when it does not exist in the United States.* Infection, it may be assumed, is a *subordinate cause* of the propagating of malignant distempers; but is in itself an *effect* of some more general cause, whose force is a hundredfold more powerful and formidable than that of infection.

If infection were the principal and specific cause of its propagation, it must rage forever, or as long as any of the human race should survive to receive it; for the longer the disease exists, the more extensive it must be. *This conclusion is inevitable;* for if infection spreads the disease so rapidly, that one or two diseased persons diffuse it in a few weeks over a city, the same principle must, in a given time, extend it over the whole earth, unless its operations should be arrested by a *superior* cause. And as one diseased person is supposed to infect more than one healthy person, its progress must be accelerated, in a duplicate, triplicate and quadruplicate ratio to its distance from its source. Its velocity also must be increased, not only by numbers in a crowded city, but by an augmented virulence, until all the inhabitants should be destroyed. *But all this is contrary to fact.* Since the plague spreads with astonishing rapidity the inference is drawn that the epidemic is the effect of some insensible qualities (probably electrical) in the atmosphere.

For one who had no knowledge of actual causes of diseases, Webster offers this remarkable distinction between "Contagion" and "Infection." "Specific contagion I define to be a quality of disease which within a suitable distance, communicates it from a body affected with it, to a sound body, with great certainty and under all circumstances of season, weather or situations. This contagion is of two kinds; first, that which acts by contact; and which may excite the disease in the same person more than once, is certainly a very distinct species of contagion, from that of the smallpox and to this I give the denomination of *infection.*" As pointed out by Winslow, Webster recognized as contagion the only two common diseases due to filterable viruses which are apparently air borne over

a radius of some feet—the two in which neither well carriers nor infection by fomites are known.

Again: the evidence of the origin of the yellow fever in America for example, leaves no room for controversy. No intercourse existed, in 1618, between this continent and the West-Indies; nor did a single vessel pass between New England and the islands, till 21 years after that pestilence which struck the Indians in New England. Not one of the islands was then settled, except by Spaniards, with whom our ancestors had no commerce. Not an European was among the Indians, except a French seaman, who had escaped from a wreck a year or two before, and Mr. Vine's men, who arrived directly from England. These men escaped the disease, none being attacked by the Indians, another evidence of the origin of the malady in the country.

It is important to draw attention to the fact that Webster was a pioneer in the appreciation of the value of statistics at a time when correlation and probable error were not commonly used. To obtain the necessary statistical data he requested information from all medical and philosophical societies to be communicated to him under the following heads:

The time of the appearance and disappearance of any epidemic disease, with its general history.

The places where it first occurs to be described, in regard to land and water, height of the land, construction of the city or streets, position as to points of compass, woods, morasses, &c. The classes of people most generally affected.

The general state of the seasons, as to heat, and cold, drought and moisture.

The time of earthquakes, meteors, lumen boreale, and all singular celestial appearances—with unusual tempests, especially when accompanied with hail—all compared with the lunar phenomena.

The appearance of unusual insects of all kinds, and any circumstance attending them.

Diseases among cattle, sheep and other animals.

Sickness and death of fish of all kinds.

Volcanic eruptions, with the phenomena preceding, attending and following them.

What was back of Webster's mind in seeking such information? He believed that if it were known that pestilential fevers never appear in the temperate latitudes, without certain *precursors,* physicians could *foretell* the danger and prepare for it; if, in certain years, pestilential fevers are universal over sea and land, necessary precautions could be taken in fleets and merchant vessels; if pestilence is *progressive* the inhabitants of large cities could undertake prudential measures beforehand and rigid execution of health laws avoided unless definitely warranted; especially would such information be useful to guard public health from the ill effects of bad provisions regarding packing and repacking of juices, fish, and flesh. In this way Webster creditably discharged his responsibility as a lay citizen in the important field of public health. In this manner, Webster, as the first historian of epidemic diseases, developed the Hippocratic concept of the "Katastasis Loimodes"[21] and was motivated in the best tradition by the heart as well as the mind.[22]

Webster was also a pioneer in stressing the need for informing the public concerning matters of public health. "Medical gentlemen," he wrote, "will excuse me for expressing ideas which belong more properly to their province. They are not new to that description of citizens, but, if just, they ought to be *universally known*; for they lay the best foundation for a regular plan of economy in domestic life which will greatly alleviate the distresses of autumnal epidemics." In this effort to keep the public informed Webster found many an occasion to criticize the members of the medical profession. He was outspoken in the matter of the rigidity with which physicians were blinded by the prevailing systems of medicine; he attacked Mead and other eminent physicians for not taking the pains to introduce improvements into the arrangement and structure of our cities which could, in his opinion, have secured our citizens from nine-tenths of the infectious diseases. He further declared that while ignorance of the nature of a disease and its degree of danger may be pardonable in a prince or the legislators of a free state, it was a crime for medical professors to be so ignorant. It may be charged that Webster over-extended himself in such criticisms; nevertheless, his active participation in hurling such shafts led to further investiga-

tions on the part of the medical profession to find the truth based on facts.

Letters to Dr. William Currie [8]

The contents of these letters are important in any appraisal of Webster's contribution to American medical thought, if only because they formed the basis and plan for his "A Brief History of Epidemic and Pestilential Diseases." They are valuable as an example of an intelligent layman's challenge of the opinions held by an eminent physician of Philadelphia, Dr. William Currie. The letters are written in an attractive, conversational style, although they lack the organic quality of historical presentation. They show a keen analysis of the problem and give a meaningful picture of the origin and spread of epidemic disease. In them, Webster shows himself a true social scientist.

These letters appear here for the first time since their publication, in 1797, in the October, November and December issues of the (*New York*) *Commercial Advertiser*, which are to be found only in the Library of Congress, the Boston Athenaeum, the Philadelphia Library Company, and the New York Historical Society.[10]

When yellow fever appeared in Philadelphia in 1797, Dr. Currie, a highly respected physician, undertook to inform the public regarding the nature and origin of the disease, by addressing letters to Mr. Benjamin Wynkoop through the daily newspaper. In particular, Dr. Currie wished to prove that yellow fever is propagated by contagion, that is, by the effluvia of the human body, conveyed by the air. Webster carefully read these Communications and decided to reply to them. Such an undertaking required courage, especially when it is remembered that Dr. Rush had already written to Webster to the effect that "Currie is now the oracle of our city upon the subject of yellow fever."

There were several reasons which impelled Webster to answer Dr. Currie. In the first place, he wished to demonstrate to the general public the fact that the question of importation, or domestic origin of yellow fever, rested principally on fact and not on medical skill; second, he designed to prove that yellow fever was of local origin; third, he believed that the discussions and investigations of

this problem came properly in the domain of laymen as well as of physicians; fourth, he felt the necessity of diffusing such knowledge of the cause of disease as was then available to the public at large, believing such information to be of the greatest importance to their health and happiness; lastly, he had the conviction that to be silent on a subject involving the fate of millions yet unborn was criminal.

Webster's friendship with the scholarly Dr. Benjamin Rush influenced his thought and writings. Some of the correspondence pertinent to this paper follows:[7]

Rush to Webster:

On November 27, 1797: They [the letters to Dr. Currie] have made a greater impression upon our citizens than any thing that has ever been published in Philadelphia upon that subject. I hope you will preserve your excellent letters in a pamphlet for the benefit of all the States in the Union and of posterity. I consider you as my advocate with the citizens of Phil[a], for the torrents of abuse lately poured out against me have been received by them with pleasure or indifference. Chiefly because I have maintained, and propagated the hateful idea of the domestic origin of yellow fever.

The optimism of Rush did not bear fruit and the pamphlet failed to be published.

Rush to Webster:

On December 29, 1797: All those Physicians who believe in the domestic origin of the yellow fever, and who pursue *nearly* the same modes of practise, have lately formed themselves into a medical society for the purpose of promoting medical science untrammeled by the systems of medicine which now govern the greatest part of the Physicians in our city. We expect to publish a volume of transactions annually. Your letters to Dr. Currie will form a part of our first publications. I communicate this information thus early to you, that you may not take any steps to have them published in New York.

Here again, Rush met with disappointment, for the medical society established was short-lived and the letters were never published. The same letter continues:—

The College of Physicians have decided, it is said, against us in favor of the importation of our late fever. I do not find your letters have made any converts. But they are a seed, the fruits of which will appear hereafter. There are not more than 20 persons in our city who have admitted our opinions. They are often opposed not so much with the resentment, as with

rage. I tremble in contemplating the fate which impends the ignorance and obstinacy of our citizens. I have made a last effort to serve and save them. Hereafter I shall sympathize at a distance with them in their sufferings . . .

The tone of this letter is in sharp contrast with the optimism expressed in the one dated November 27th, especially with reference to the number of converts.

Rush to Webster:

On March 30th 1798: You have opened a mine of precious metals to the lovers of science and friends of humanity. I have for some time past suspected that the malignant constitution of the atmosphere taken notice of by Hippocrates and Dr. Sydenham pervaded our *whole* globe, at the *same* time and that it was somehow produced by the influence of some part, or parts of the solar system upon our earth. But you have demonstrated that to be true which was only a floating idea in my mind . . .

This is a striking tribute by an eminent physician to the efforts of a layman who tried to establish the cause of disease.

Rush to Webster:

On April 27, 1798: The Academy of Medicine have published their proofs of the domestic Origin of our late Epidemic. It cannot be refuted, but it will not produce in our city such an effect as to clear a single gutter of its filth. I fear nothing but another visitation by the yellow fever will cure us of our infidelity upon this interesting Subject. Go on—go on with your inquiries. Cause Physicians to blush and instruct mankind to throw off their Allegience to them. Posterity will do you justice. The man who produces truth upon the Origin of pestilential fevers, and persuades the world to conform to it, will deserve more of the human race than all the Heroes, or Statesmen that ever lived.

Rush's indignation did not deter him from urging Webster to continue his efforts in behalf of truth.

Webster to Rush:

On December 4, 1798: I observe a Committee of your citizens propose to petition Congress to prohibit the arrival of vessels from the West Indies and Mediterranean during the summer months. I can make some apologies for folly in the peculiar circumstances, but in this proposition is something worse than folly. It is a serious attack on the commercial interests of the country. I can ascribe it only to insanity for you are sensible that madness is often epidemic after pestilence.

Webster to Rush:

On February 27, 1799: The evidence I have collected, (A Brief History) will *demonstrate* that the *great primary* cause of epidemics, is a quality of the atmosphere, extending *usually over one hemisphere*, sometimes *over the* globe. This quality will probably appear by its effects to be *superabundant* stimulus. The mass of evidence to prove the *fact*, defies opposition and it will render the cause probable. In a few instances, this general cause, with the season, has produced true plague, without the least operation of noxious exhalations. In most cases, however, the plague is not produced without the cooperation of local vitiation of the air.

Rush to Webster:

June 20, 1799. I have made a second unsuccessful attempt to dispose of your book. Our printers and Booksellers (one excepted) are among the believers in the importation of the yellow fever, and hence they cannot easily be persuaded duly to appreciate your opinions which are universally known to be on the other side of the question. The prejudices against the truth upon this subject, are as strong as ever. Currie in a late publication has attempted to prove the assertion of the Domestic Origin of the yellow fever to be equal to treason, and has advised that the authors of it should " no longer be tolerated." Cobbett after cussing our climate for two years for generating the yellow fevers, now conforms so far as to the current opinions of its foreign origin, as to abuse me publicly for denying it. In short, some Demon seems to have blinded our criticism, as if to prepare the way for their future and greater sufferings from that disease.

After this information I hope you will not hesitate in sending your book to London to be printed. I will give you an introduction to Dr. Lettsom.

Webster to Rush:

On November 26, 1799: I feel a good deal of confidence in my proofs that epidemic diseases of all kinds proceed from chemical changes in this fluid in which we live and whose vivifying and elastic powers constitute the source of life . . . That the electrical principal is the agent, I am inclined to believe; indeed, I have not the power to resist the evidence of it; but by what combination with aerial substances, I am ignorant.

Rush to Webster:

December 1799: Accept of my thanks for the instruction I have derived from your history of Epidemics which Mr. Cruickshank presented to me in your name. I have seldom read a book with more attention or deliberation. I have even gone over several chapters of it twice. I have nearly finished such an index to it as will enable me to lay my hands upon the most important facts contained in it at any time when I require them.

It is of special interest to observe that Rush had begun to make an Index of this work, the lack of which, in a book otherwise remarkable for its sweep of facts, was one of its major weaknesses.

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WEBSTER'S LETTERS TO CURRIE IN THE NEW YORK COMMERCIAL ADVERTISER *

LETTER I (Thursday, October 26, 1797)

To Dr. William Currie:

The zeal and public spirit, displayed in your letters to Mr. Wynkoop, of the 2d, 5th, and 10th of October, cannot fail to be acknowledged by all your fellow citizens, and the immense importance of the subject you have discussed will recommend the letters to a general reading.

Your own sense of the magnitude of every question that concerns the origin, nature, prevention or cure of the malignant disease which afflicts Philadelphia and has afflicted other towns in the United States, will excuse a private citizen, who is a stranger to you, and to the medical profession, for addressing some remarks to you on the subject you have so elaborately discussed before the tribunal of the public. Truth and the public good are doubtless your sole objects, as they are mine, and of course no private views or passions can mingle themselves in the discussion and give an improper bias to our inquiries.

You begin your letters by certain positions, which are considered as *data*—viz. that *vegetable* effluvia from low marshy grounds produce intermitting and remitting fevers, and these fevers are never contagious; but that *human* effluvia produce continued fevers of a nervous or putrid kind, which may be and usually are contagious.

I frankly own that I am not convinced of the truth of these positions, in the utmost latitude; but for the present, I shall waive the discussion of these points, and for the sake of argument, admit *your* principles, and the authorities you have cited. However, I cannot admit your conclusion from those premises, that the contagious yellow fever, is not the production of our climate, but always imported from the West Indies and propagated only by contagion. I deduce a contrary conclusion from the very facts you have stated.

The first fact is, that six recovered marines from the Hospital, entered on board the Loestoffe, at Quebec, in 1758, to take passage for England, and infected the crew of the ship with a malignant fever. This is an instance to verify one of your first principles, that malignant fevers proceed from human effluvia. But I am surprised that you should, after stating the fact, conclude that such fevers are not produced in our northern climates. My conclusion, on the contrary, is that if human effluvia will generate a contagious fever in the latitude of 50°, a fortiori, will human effluvia produce a similar disease in the latitude of 40°.

* Webster's original spelling and punctuation have been retained throughout.

If the crowding of men together in the close holds of ships did in fact generate a malignant and contagious fever, (call it yellow or spotted fever or what you please) on their passage from America to England, in the latitude of Canada and England; what is to prevent crowds of men, stowed in close rooms, in dirty narrow streets, where no fresh air can reach them, perhaps drinking impure water, and wallowing in filth, from generating a similar disease in New York or Philadelphia? I appeal to your own logic for an answer.

You may perhaps say that it is not proved that the fever originated on board any of the ships; and that it is most probable the contagion was communicated to *all* the infected ships, as it certainly was to *some* of them, from the hospital at Point Levy, near Quebec. Admitting the fact, how came the fever in the hospital in that climate? There is no evidence or pretence that the contagion was originally conveyed from the West Indies or from Africa, and propagated among the troops. On the contrary, it appears that the disease was generated among the troops in Canada.

If then the fact is proved, that a contagion may be generated by human effluvia in the northern climates on this continent, surely it is not impossible nor improbable that a contagion may originate in the more temperate climate of the middle states.

The circumstances of the French fleet which sailed from Brest to Louisburg in 1757 and returned to Brest, with a malignant fever among the troops, ought not to pass unnoticed. That fatal disease, which destroyed ten thousand men, either belonging to the fleet or to Brest, originated either in Brest, at Louisburg, or in the fleet on its passage; and in either case, the latitude in which the disease arose, is far north of the climate of the middle states.

Innumerable similar facts, well known to a gentleman of your professional reading, might, one would think, forever silence the lips of those who talk of driving the plague and other malignant fevers from some particular climate, and restoring to our country its *reputation for healthiness.*

Nothing is more demonstrable than that the atmosphere of a city, as well as the air of a jail or the hold of a ship, may be so vitiated, by the volatile acids evolved from animal and vegetable substances, as to be unfit for the support of healthy life. And this is true in all latitudes, where men live, tho it may require a less quantity of those substances to vitiate the air in the torrid zone, than in the temperate, and less in the temperate, than in the frigid.—On your own principles, you must admit that wherever there exists animal and vegetable substances, there exists the sources of bilious and malignant diseases. There requires nothing more to put these causes into operation, than a sufficient degree of heat to extricate the acids from their substances, and to expose men to their action. And it belongs to you, Sir, to prove that there is not heat enough in Summer any where to the southward of the 60th degree of latitude, to decompound those substances and render the poisonous effluvia, active and fatal.

The high northern latitudes are certainly *less* exposed to malignant autumnal fevers; for the causes of them exist there in a much less degree. The summers are short, and few large cities exist in the high latitudes. The extreme heat is of a very short duration—it is frequently interrupted with cold northerly winds and what is more, the violent heat of the day, in all northern latitudes, is interrupted by cool refreshing nights.—In such climates the human body rarely becomes so debilitated, as to suffer much derangement of its functions from heat. Still in all the habitable parts of the world I maintain, that the causes of pestilential disease may exist and produce their effect.

I would further observe, that the temperate climates which experience alternately great heat and extreme cold, are precisely those which have most to fear from the plague, or yellow fever. In this doctrine I am warranted both by theory and fact. The reason is obvious.

In the torrid zone, natives of the climate are fitted by nature and by habit to bear continued heat, and the operations of heat upon objects around them.

Hence, the natives of Africa and the West Indies, are seldom affected by the plague, or yellow fever. The same is true of the Spaniards in the unhealthy regions of Carthagena and Porto Bello. The ordinary yellow fever, which seizes our brethern who go to the West Indies on trading voyages, rarely or never touches the natives of those Islands.

It is not so with the Inhabitants of the temperate climates. Here we pass, twice in every year, thro all the varieties of temperature from below 0 by Farenheit to 93 or even to 98° above. Our bodies are not fitted by habit to sustain continued heat; and we are exposed, in passing from winter to summer, to precisely the same inconveniences as the man who passes from a northern to a southern climate—a change which uniform experience proves to be hazardous to health.

In the frigid zones, the heat, being of short duration, and interrupted, does not expose men, in the same degree, to the diseases of a bilious and malignant type.

This is probably the reason, sir, why the most malignant of all diseases, the plague, or worst form of the yellow fever, has, from the earliest dates of historical records, been *most common* and *most* fatal in the temperate zone. Yes, Sir, contrary to your deductions, you and I live in the very climate, which, by the laws of nature, is doomed to be the *most subject to the plague.*

This, is a most serious consideration, and for this reason alone, I am determined to devote a few hours to an examination of the doctrine of foreign origin, which I believe to be an error—a doctrine, which like predestination among the Mahometans, bodes more calamity to these delightful regions of freedom and peace, than all the wars that afflict Europe, and all the physical

evils of frosts, tempests and earthquakes. My reasons for this opinion will appear in succeeding letters.

I am, Sir
With great respect,
Your obedient servant,
NOAH WEBSTER, Jun.

New-York, October 1797.

LETTER II (October 28)

In your second letter to Mr. Wynkoop, you have given a lengthy extract from Dr. Chisholm, with the view of proving that the pestilence which ravaged Philadelphia, in 1793, was not the common Yellow Fever of the West Indies, but a more malignant and contagious distemper, introduced into Grenada by a ship from Boullam, on the Coast of Africa, and from thence communicated to other Islands and to the United States.

All this may be true; but that such an extensive conflagration in the West Indies, and the United States should have been enkindled by a single spark from Africa; is a fact that must be fully proved before it will command universal belief. I shall not, however deny at present the fact; but I think it capable of demonstration, that neither that disease nor any other plague from Africa can be spread in the United States without auxiliary causes *originating* and *existing* in our country, independent of imported contagion.

I firmly believe we have facts enough on record to convince the most obstinate sceptic, that whatever be the truth as to the *first introduction* of the plague, the communication and prevalence of it as an epidemic disease, depend wholly on the constitution of our own atmosphere. If this point should be admitted, and I have never found a man to deny it, it is all that is necessary to my present purpose.

I have the pleasure, Sir, to be well acquainted with Mr. Phiba, the gentleman from whom Dr. Chisholm obtained his information, relative to the expedition to Boullam, and I will, with pleasure, introduce any medical gentleman to him in this city, for the purpose of gaining further and more particular information on the subject of the disease which swept away two fifths of the people, who embarked in that enterprize. His relation of facts, to me, corresponds in a degree, with the account given by Dr. Chisholm, but it is not his idea that his ship introduced a new or any disease into Grenada.

But let me ask, Sir, how and by what means did the disease at Boulam originate among the people? Did it owe its origin to contagion derived from any person or thing at Boulam? No, certainly; for it is an uninhabited

island, and has no marsh grounds to vitiate the air. The inhabitants on the neighboring continent could not infect the people, for they knew no such disease. We come then to the true origin—a plague, a deadly plague appeared among the people without any local cause, peculiar to the climate, except heat and moisture. This inference appears to be undeniable; that *contagion was not the origin* of the disease among the English people. The people themselves, to use the vulgar, but expressive phrase, *bred the plague* among themselves. The distemper was *not* the distemper of the climate, as peculiar to it, not of the natives of the climate, for they are strangers to such diseases. It was the disease of the people who had changed their climate, without a corresponding change in their constitutions and their aliment. The change of climate was sudden, whereas all changes in the habit of the body must be gradual, or they will be deleterious.

Add to this powerful cause, the animal effluvia, which must have been generated in superabundant quantities, on board the ships where they were confined.

Here then we have one remarkable instance of a plague originating incontrovertibly without specific contagion; and I do not believe Sir, it is in your power to produce another example so directly in point, to overthrow your own doctrine, of importation into this country.

The circumstances of this case, tho they happen on a smaller scale every year to the crew of vessels going from northern to southward latitudes, are so prominent that they deserve particular notice, as marking, with precision, the peculiar danger to which the inhabitants of temperate climates are exposed, every summer. They are circumstances analagous to those we all experience, in passing from winter to summer, every year of our lives.

I repeat the remark and dwell on it, because eminent physicians, with the best intentions no doubt, are constantly sounding the praises of OUR CLIMATE in point of salubrity, while we are more subject to pestilential autumnal fever, than the natives of the torrid zone. I repeat it: we change our climate, so to speak, every year; we pass from the frigid to the torrid zone, as it respects the temperature of the weather. We annually run the risk of the settlers at Boulam, of *generating a plague* in all our large cities, and if we do not experience the dreadful calamity, it is for two reasons:—First, the change of weather from winter to summer is less rapid, than the change of climate by a vessel going from a northern to a southern latitude; by which means, the human body, in a degree, accommodates itself to the change, and is prepared to resist its effects. Secondly, the heat of our climate tho as intense as in the torrid zone, is not always continued, but often interrupted with cool northerly winds, even in the summer. These favorable circumstances will often save us from malignant epidemics. But whenever long continued heat invades our climate, the human body, not being inured to it or fitted, like the natives of the torrid zone, to resist its effects, sinks under its operation; and the local causes of disease, the effluvia from vegetables and animal substances,

always existing in our large cities, will aid in kindling and spreading the flame.

And these local causes, Sir, are rapidly increasing. Our winters are evidently becoming more generally moderate. If we have as much snow as formerly, which may be true, the frost is less uniform and severe. Our commercial cities are growing with astonishing rapidity. In one century, the continuation of their present increase, will place New York and Philadelphia, in point of numbers of inhabitants, on a footing with the first rate cities of Europe and Africa. And now is the time, Sir, to provide against the annual ravages of the plague hereafter, without the necessity of burning and rebuilding our cities.

P. S. In the following paragraph of my first letter, "Hence the natives of Africa and the West Indies are seldom affected by the Plague or Yellow Fever"; after *Africa*, add the words "in the Torrid Zone."

LETTER III (October 30)

Hitherto I have reasoned on *your* premises as if well founded. But, Sir, I doubt much the truth of the principle you have advanced, that bilious and malignant complaints, arising from vegetable effluvia alone are *never infectious*; there are many facts to overthrow that position.

The schooner Huntress, Captain Wasson, arrived in New York harbor, from Savannah, not three months ago, with bags of cotton on her deck. All the crew were in perfect health, when the vessel left Savannah. The weather was excessive hot, and the cotton was wet with rain. The winds were light, and before the schr. arrived four of the people were taken ill—one died covered with large carbuncles, some of them black, the distinctive marks of a most virulent plague—and another had them on his forehead as large as nutmegs.—These men were mostly on the main deck; and the master, who slept in the cabin, did not escape without inconvenience from the cotton. On removing the bags, it was found the fermentation of the wet cotton had rotted the canvass, so that it gave way and the bags fell in pieces by removal.

The health officer of the port will state the case at some future time, with more precision. I have given you the substance and leave you to determine how far *human* effluvia had an agency in producing the plague on that schooner, and whether the sick men, introduced into the city would not have endangered the lives of the inhabitants.

There are but two ways of evading the conclusions that vegetable effluvia were the direct exciting cause of the disease, and that the disease was as infectious as any fever that appears in our climate. It may be said, the fever was nothing more than the effect of fatigue and exposure to the weather, or other common causes; or it was only an ordinary bilious remittant.

But the disease of the seaman that died was marked with most malignant symptoms; and if those who arrived sick, had lighter complaints, this only proves that the disease of our summers assumes every grade of violence from ordinary remitting fever to the most fatal forms of the yellow fever.—This is a fact, that a mortal disease *did* originate in a vessel on our coast, and whether from the cotton or from other causes, it proves there is nothing in our climate to exempt us from the fate of other nations in the same latitudes.

It will be proved in a subsequent letter that the fever at Boulam which you alledge to have been a more fatal disease than the common yellow fever, did, in fact, exhibit at that place, all the varieties of bilious fever from the very plague that takes a healthy person to his grave in 24 hours, down to a common intermittent, that lingered for months. And when the Hankey arrived at Grenada, no man was sick aboard, nor had been for a long time, except Capt. Cox, who had only a lingering irregular intermittent.

But at present; I shall not pursue this inquiry. Whether the sources of *pestilence* are in animals or vegetables, may not be an inquiry of so much consequence; as it is agreed on all hands, that the effluvia from both substances in a state of putrefaction, are highly pernicious to health; and it is hardly necessary to remark that all animal substances are directly or remotely derived from vegetables, and of course are but different modifications of the same elementary materials.

It is to be wished, Sir, that the words *infection* and *contagion*, had each a definite technical meaning. In ordinary language they are used as synonimous, and so are they explained in dictionaries.

Indeed the originals will well enough warrant this promiscuous use; *Infection* being derived from *inficio*, signifies conveyance from one thing to another by introduction, infusion or contact—and *contagion* from *contingo*, is something communicated by the touch.

But could a distinction be established corresponding with a difference which really exists between the natures of different diseases, in respect to their capacity of being communicated from person to person, men would be less liable to mistake each other and to dispute about words.

I shall take the liberty of using these words, according to the difference of meaning established by Dr. Bayley, in his account of the yellow fever of this city. By a *contagious* disease, I mean one which may be and usually is communicated from person to person, by the touch or near approach, independent of season or accidental circumstances; as the small pox, measles and hooping-cough.

An *infectious* disease I consider as one capable of being received from diseased persons *only* under certain circumstances. Thus the malignant yellow fever or plague is often communicated from person to person but only under circumstances of crowded population, hot weather, close rooms, &c. Frost destroys its powers of propagation by contact or near approach and in

a thin population and salubrious air, the disease disappears. The contagion of the small pox, measles &c, I call *specific*. The yellow fever or plague has not this quality of *specific* contagion, but is *infectious* and may under certain circumstances, become an epidemic by *infection*.

That this distinction is just one, will appear from facts, collected from historical records, and from daily observation in our own country. To prove this point, and that the origin and communication of the plague depend *not on specific contagion*, but on local or accidental circumstances, it is necessary to recur to all the evidence we can collect either from ancient or modern times.

To do justice to the subject, would require more time and materials than I have at command, but such authorities as fall within my power, without much labor and research, shall be presented to view.

As a preliminary, however, it is necessary to determine, whether the yellow fever and the plague are specifically the same disease. This, I believe, is admitted by physicians in general; and I find it your opinion also that the plague is only the highest grade or worst form of yellow fever.

But it is remarkable how exactly the plague at Athens, four hundred and thirty years before the christian era, resembled the malignant fever, which annually rages in some of our sea ports. The resemblance ought not to be overlooked, and I shall transcribe the symptoms from an English translation of Thucydides, the most faithful historian of Greece, who was himself affected with the disease. The following are his words:

"Those who enjoyed the most perfect health were suddenly, without any apparent cause, seized at first with headaches extremely violent, with inflammations and fiery redness in the eyes. Within, the throat and tongue began instantly to be bloody; the breath was drawn with difficulty and had a noisome smell. The symptoms that succeeded these were sneezing and hoarseness; and not long after the malady descended to the breast, with a violent cough. But when once settled in the stomach, it excited vomitings, in which was thrown up all that matter physicians call discharges of the bile, attended with excessive torture. A great part of the infected were subject to such violent hiccups without any discharge, as brot upon them a strong convulsion. The body was not very hot to the touch, nor of a pallid hue, but reddish, livid, and marked all over with little pustules and sores, yet inwardly scorched with excessive heat."

The historian then specifies the "unabating restlessness," which tormented the sick. The disease terminated fatally on the *seventh* or *ninth* day; or if the persons survived these periods, the distemper fell into their bowels and caused violent ulcerations, accompanied with an excessive flux. Some persons lost the use of their limbs—some their eyes—and some their memory. so that they forgot their friends. One of the most affecting circumstances, says the historian was, the dejection of mind which attended the first attack—the mind sinking into despair.

Let any man compare this account of the Athenian plague, with the symptoms of the yellow fever, as described by Dr. Rush, Dr. Bailey, or Dr. Smith, and deny, if he can, that the yellow fever of our country, is the specific disease which Thucydides described more than 2000 years ago. Indeed whatever small varieties the disease may exhibit in Asia, Africa or America, it must be agreed, on all hands, that the principal characteristic symptoms of the plague and the yellow fever are the same. I shall therefore call the disease which has afflicted our sea ports the *plague*—that being the technical term in common use to specify the worst form of bilious fever. It does not indeed in most instances, assume the virulence and activity that it is said to do, in certain cities of Africa and Asia. But I am assured, that in numerous instances, its malignancy is equal to any thing related of the plague in foreign countries.

My general position is, that those prominent symptoms of the yellow fever, which *invariably* attend the disease in the United States, are all found in the plague in Africa and Asia. The varieties of subordinate symptoms, which are observed in different patients in this country, are merely accidental; depending on varieties of situation and constitution. The same varieties are observed in the plague in all countries, and form no objection to the identity of the diseases.

LETTER IV (November 1)

To prove that the plague, tho infectious, does not depend on specific contagion for propagation, but on accidental and local circumstances, let me call your attention to facts.

The first accounts we have of the plague are in scripture. History, sacred and profane, speaks of this disease as existing in Egypt, as early as the dates of the earliest records. We have no accurate description of its symptoms, in those early times; but we are informed of its great mortality, and that the Egyptians, and Israelites made use of fire to arrest its progress. But we may fairly presume, that the disease in ancient times was substantially the same, as it appears to be in modern times. Let us then examine why the disease has been and is still a common malady in that country.

Egypt is situated between the latitudes of 25 and 30 degrees north; a climate corresponding with that of Georgia and Florida on this continent. The country is penetrated by the Nile, and inclosed on both sides, for the greatest part of its length, with mountains or ridges of high lands. In some places, the high lands approach within fifteen leagues of the river—in other places, they are distant 50 leagues. The space between the mountains is flat land, which in Lower Egypt, is every where penetrated with canals, for the purpose of conveying water from the Nile into all parts of the adjacent plains. A great portion of the low country is annually overflowed

with the waters of the Nile, which descend from the mountains of Abysinia in May and June, and do not subside till October and November. There is no rain in Egypt; the people depending altogether on the Nile for refreshing and fertilizing their lands. No sooner has the flood subsided, then the earth is exposed, for five or six months, to a cloudless burning sun, which must act, with great force, on the mud and slime, and finally dries and parches the wet plain. Even the numberless canals, which are reserved for water, often become exhausted.

It is hardly possible, Sir, to conceive more of the elements of bilious complaints to be scattered by nature and art over the face of a country. Instead of being surprized that the plague is so frequent in the populous parts of Egypt, we should rather think their exemption from it a miracle. Yet even in Egypt the plague is ascribed to *imported infection*; and no less an author than the philosophical Volney,* denies the disease to be of Egyptian origin. He quotes the "European merchants who concur with the Egyptians in declaring that the plague never proceeds from the interior parts of the country, but first appears at Alexandria, then passes to Rosetta—then to Cairo and thence to Damietta and all over the Delta. They find further that the plague is always preceded by an arrival from Smyrna or Constantinople." Travels in Egypt and Syria, vol. 1, 253.

All this, Sir, may be true; and it is generally true of the plague in all other countries. It not only *begins*, but usually is *limited* to the shores of the sea and of rivers. And it is precisely the case with the yellow fever in the West Indies—it begins and rages in the seaports, and about the harbors. But will it be inferred from thence that the yellow fever is always imported into the West Indies?

One circumstance alone, related by this writer might have convinced him that the disease in Egypt is of domestic origin; this is, that the plague never rages in that country in summer, but in winter. The reason of which is, that the plague or some milder form of bilious fever, follows the fall of the Nile in October and November. The heat of that country, during our winter, is equal to our ordinary summer heat, and of course adequate to the production of bilious complaints. The plague appears in a few weeks after the sun acts upon the earth which is left *moist* by the retiring waters of the Nile. Nor ought we here to omit mentioning the striking analogy which this fact exhibits between the plague and the intermitting fever in our country. It is a prominent circumstance, in the knowledge of every man of observation, that flat and marshy lands, suddenly left uncovered by water, if the weather is hot and the lands of considerable extent, invariably produce the intermitting or remitting fever, or both. Perhaps there is not in the physical world any one effect that more certainly follows its cause. This circumstance alone establishes the alliance between our bilious remittent, and the plague—And

* So do the Baron de Tott and Savary.

what, let me ask, is wanted in our climate to convert our bilious remittent, into its more malignant form, the plague, but the circumstances of immense masses of filth, and crowded population, to generate human effluvia? I submit the question to your better judgment.

Now if the plague in Egypt is always imported, and not generated in the country, why should it not be imported in summer as well as winter? Vessels from Constantinople, or from Smyrna or other infected places may and do arrive at the ports of Egypt at all times of the year. It is impossible then to ascribe its origin and propagation to imported infections; for if it depended on this, its appearance would be irregular and accidental; whereas its appearance and disappearance depend on the seasons. A hot sun, acting on an immense mass of filth, deposited over the face of the country, by the waters of the Nile, excites into action the effluvia which destroys health. Then the plague appears. After the continuance of heat has dried the whole country, and putrefying vegetable substances no longer supply the effluvia, a principal source of pestilence is exhausted, and the plague disappears. The regularity of this phenomenon leaves no question as to its true cause.

But it must be observed, that the plague, even in Egypt, commits its principal ravages along the banks of rivers and in populous cities—another striking resemblance in that disease to the plague in the United States. This fact, which seems to be uniform in all countries, appears to warrant the supposition, that animal and vegetable effluvia, and a moist atmosphere, all combine to produce this disease in its most deleterious form.

But even admitting that imported infection is always the original and exciting cause of the plague in Egypt, yet one conclusion remains indubitable, that its *propagation* depends entirely on accidental circumstance—viz. the *season of the year*—a fact that demonstrates my first position that the contagion of the plague is not *specific*, in the sense before explained.

Mr. Volney mentions that the Egyptians are of a bilious habit—they are troubled with the acid nausea, and the cholic is their habitual malady. This circumstance confirms my opinion of strength and number of causes in that country, predisposing the body to bilious fevers.

He observes, further, "that malignant fevers sometimes become epidemic, in which case they are easily mistaken for the plague." This fact proves that bilious fevers, specifically the same, are distinguished by different degrees of virulence and activity; and that the shades of difference are sometimes, scarcely visible.

I cannot close this letter without observing how apt men are to save themselves the trouble of reflection and inquiry, by resolving phenomena into visible causes.—"Some vessel, say the Egyptians, always arrives from Smyrna or Constantinople, just before the plague appears." They instantly conclude that such plague arrives in that vessel, without examination or evidence, because they can see and feel a ship, and they cannot see or feel

the deleterious quality of the air, which is now known to be the cause of our worst distempers. It is more easy for the mass of men to take the evidence that readily presents itself to the senses, however superficial and fallacious, than to enter into researches that require the exercise of the understanding.

The habit of ascribing the origin and prevalence of infectious diseases in Egypt to importation, resembles very much the vulgar opinion of the *sun's drawing water* from the earth to the clouds, as a forerunner of rain. Now the streaks of light and shade which people suppose to be water rising to the clouds, are in fact merely the light of the sun shining through the thin clouds and vapor in the atmosphere. It is true that this phenomenon indicates approaching rain; not because the water is rising in streams, but because this appearance denotes an atmosphere highly charged with vapor. The vulgar error, however, descends from generation to generation, and so it will, while most of the human race are governed more by their senses than by the deductions of reason.

One remark more. The Egyptians, Sir, deny the domestic origin of the plague.—They are doubtless jealous, like many of our citizens, of the reputation of their country for healthiness; they therefore ascribe the disease to infection from Smyrna or Constantinople. Is it not really strange, Sir, that the Egyptians should insist on the generation of that disease in a *northern* climate; while you and many worthy and learned American citizens, should contend that it originates only in *southern* latitudes? Surely, Sir, there is something wrong in the current opinions of the Egyptians or of the Americans? If climate has any concern in giving rise to the disease one of the forementioned opinions *must* be erroneous. I believe them both to be wrong.

LETTER V (November 2)

Let us, Sir, examine the subject of the introduction of the plague into Greece, Rome and other countries bordering on the Mediterranean.

The first plague in Greece of which history has prescribed us a clear account, is that already mentioned, which happened in the time of Thucydides, during the Peloponesian war, 430 years before the christian era. Of that terrible calamity, Thucydides, the most minute and faithful historian, has left us a circumstantial description; on which Dr. Elihu H. Smith, of this city, has lately written and published, in the first volume of the Medical Repository, a very ingenious and interesting comment. This account is extremely important as the disease described bears a very exact resemblance to the yellow fever of this country. How then was this plague introduced into Athens?

The Abbe Barthelemy, in his elegant and masterly travels of Anarcharsis, says " this disease was doubtless brought into Greece by a vessel from Egypt." It is to be regretted that such an accurate writer should have indulged a

conjecture on that subject. He quotes no authority to that point but Thucydides; whose account will not support the opinion. His words are, "It broke out first, *as it is said*, in that part of Ethiopia which borders on Egypt; it afterwards spread into Egypt and Lybia, and into great part of the king's dominions, and from thence it on a sudden fell on the city of the Athenians. It showed itself first in the Piroeus (or the sea port town) which occasioned a report that the Peloponnesians had poisoned the wells. After this, it spread into the upper city and the mortality very much increased.

Let everyone, physician or not, freely declare his own sentiments about it; let him assign any credible account of its rise, or the causes strong enough in his opinion to introduce so terrible a scene."

From the words of this most accurate historian, the deduction is clear that there was no ground to believe the infection imported. The disease appeared in Ethiopia, Lybia and Egypt, as it has in various parts of Europe since, and in the United States; but there is no authority to say it was communicated from Egypt to Athens by infection. On the other hand, it is certain from Thucydides, that if imported, the Athenians had no suspicions of it; otherwise they would have attributed the disease to its true origin, and would not have raised a report that their enemies had poisoned the wells. And the historian himself did not believe it imported; otherwise he would not have called upon any person to "assign even a credible account of its origin." The authority of Thucydides therefore is wholly against the origin by imported infection.

Plutarch in his life of Pericles, says "The enemies of Pericles attributed this disaster to the multitude of people he had collected together in the city, where they were obliged, during the heat of summer, to lead a lazy and inactive life." And this appears to have been the true origin of the plague in Athens. The Athenian Republic was invaded by the Lacedemonians, whose army the Athenians could not meet in the open field. Pericles who commanded in Athens advised and carried into effect the plan of collecting all the people belonging to the Republic into the city, and protracting the war. This unusual multitude, crowded into a city, and into all the sheds and hovels that they could find or erect, in the hot weather, generated the plague. Athens was a great jail and bred the jail fever on a large scale. The evidence of domestic origin in that city is too obvious to be resisted.

I think, Sir, it can be made probable, that the prevalence of epidemics, at particular periods, depends very much on a constitution of the atmosphere, which is visible and known only by its effects. As I intend to make this the subject of another letter, I shall only observe here, that some general state of the air favored the currency of the plague about the time of its appearance in Greece.

I have not the works of Hypocrates; but in the Encyclopedia, article Hippocrates, it is said that the great physician, hearing the plague raged in Illyrium, on the borders of the Adriatic, now Dalmatia, and knowing the

course of the winds, predicted that the disease would break out in Athens. I place no reliance on this account, even if true, further then to observe, that Hippocrates must have believed the propagation of the plague to depend on a vitiated atmosphere, rather than on infection conveyed by persons or vessels.

It never was pretended, I believe, that the Island of Sicily was, in ancient times, the hot-bed of plagues; nor is it so at this day. Yet under favorable circumstances, most deadly plagues have originated on that Island. Something more than two hundred years before the christian era, that island revolted from the Romans, and was protected by the Carthaginians. The Roman General, Marcellus, laid siege to Syracuse, which was defended by its tyrant or Governor Epydices, assisted by Bomilear, Hippocrates, (a general of that name) and Himilco. During the siege a plague broke out that made terrible havoc in the camps of the contending armies and in the city. As that disease resembled exactly the yellow fever in our country, in its origin and progress, I will give a translation of Livy's account of it. Lib. 25. cap. 26.

"In autumn there appeared a pestilence, a calamity common to all parties, which naturally diverted their minds from the affairs of the war. The place was naturally unhealthy, but the disease raged most violently without the city in the camps, where the bodies of men were oppressed with an intolerable heat. *At first the sickness proceeded from the badness of the air* (loci vitio) and proved mortal. Afterwards the care of the sick and handling them, very much spread the disorders; so that the sick at last were abandoned and died for want of care, or their attendants took it and died. At length the living became so hardened, that they ceased to grieve at the loss of friends and even neglected to bury the dead; so that dead bodies lay before the eyes of the living who were expecting the same fate. The dead infected the sick, and the sick the well with terror and contagion; so that some, preferring death by the sword rushed upon the posts of the enemy.

"But the disease was much more fatal to the Carthaginians than to the Romans, who had been long besieging Syracuse and had *become habituated to the air and the water.** The Sicilians, when they saw the disease spreading from the unhealthiness of the camp, abandoned their allies, and went home, but the Carthaginians, who found no shelter, all perished with their generals, Hippocrates and Himilco. The Romans, to lessen their danger, betook themselves to the city, where the men were refreshed with the shades of the houses. Many of them, however, perished with the pestilence."

In the foregoing account we observe the following circumstances—that the plague broke out in autumn—among the soldiers in camps—that it originated in *bad air* and from small beginnings, growing more fatal as it spread—that it was most fatal to the Carthaginians who were new recruits or had recently

* Many of the Carthaginians and Sicilians were fresh troops, and therefore they suffered most.

arrived in Sicily—and that the Romans, who had been long accustomed to the air and water, suffered less.

These circumstances are common to the yellow fever or plague at this day. Rollin, in his ancient history, vol. 10, has transcribed this passage of Livy, but has omitted one of the most important circumstances—that the reason why the Romans suffered less than the Carthaginians, was that they had been long investing Syracuse, and had become accustomed to the air and water. This fact, which uniformly characterizes the plague in this country, and in the West Indies, is material—it proves that ancient plagues were not propagated by specific contagion, but originated in *bad air* and attacked *strangers to that air or climate*, with double violence and mortality. Do we not see similar facts every autumn? Do not our northern people go to the West Indies and perish, when no disease attacks the natives? Do not our New England men die of the malignant fevers in Charleston and Savannah, when the natives of those cities enjoy health? Are not Europeans in Philadelphia, and New York the first to suffer by the yellow fever? Yes, Sir, of the 750 persons who died of the fever in New York in 1795, 600 were foreigners recently arrived. Surely, Sir, such uniform effects must proceed from the same causes—and those causes must be as permanent as the laws of the universe.

LETTER VI (November 7)

In a cursory examination of the history of Rome, I find many instances of the plague mentioned; but little or nothing said of the origin and symptoms of the disease. I regret that I have not more time and more original authors, that I could stack together all that has been left on record, relating to pestilential diseases, in that Republic.

From the short accounts of nine plagues which happened in Rome from the year 241 to the year 477, I find the following particulars: That the plague raged in the hot season, and disappeared in cold weather—that when it was violent and very general, Rome was almost wholly deserted—in one instance, the lands about the city were neglected and a famine ensued—that it attacked the "flower of the Roman youth," as now it is most fatal to sound constitutions—and that Rome was peculiarly liable to the disease in times of tranquility, when her citizens were all at home, indulging in ease and pleasure.

From these general accounts of the plague, we can only collect that the disease was the effect of hot weather and crouded population; and that the Romans sometimes abandoned the city, during the season when it prevailed, and returned when it disappeared in cool weather. In short we find enough to convince us that the disease was the same as that which affects our country.

The Roman writers mentioned one thing in which the ancients resembled the moderns, on such occasions; and that is, they had recourse to the Gods

for relief. Whenever a plague broke out, their priests consulted the oracles and Sybilline books for directions how to check the disease, and both at Rome and Athens, when they found that their prayers to their Gods were unavailing to arrest the disorder, they abandoned their devotion and gave themselves up to every species of immorality.

While I avow myself a firm believer in the being and perfections of a God, I freely declare that I do not believe *that being* ever did or will interpose a miraculous power, to change the laws of nature and prevent disease, in places where its *natural* causes exist. I do not believe that almighty power will be exerted, in a special way to cleanse dirty streets and cellars, or to save men who wallow in filth, intemperance and debauchery, from falling victims to fever.

The Roman Historians mention a most remarkable plague on the coast of Africa, about the year of Rome, 628. A violent east wind brot a cloud of locusts from Arabia or Egypt, and spread them along the coast, in such numbers that they devoured the grain and even the hardest bark of trees. A fourth wind blew them into the sea, and they were driven on shore in heaps, where they putrified and generated a plague, which spread over the country from Cyrenaica to Utica, and carried off 800,000 inhabitants of Numidia, 200,000 people on the sea coast, and 30,000 soldiers of the Roman army appointed to guard Africa. Hook's hist. vol. 2. 549. Livy, Epit. lib. 60.

I quote this instance, Sir, in confirmation of your theory that animal effluvia will produce pestilential disease. But I must contend that the same cause will produce its effects in the latitude of 40, as well as in the latitude of 33 or 34. And if an immense mass of putrifying locusts could generate a pestilence to destroy a million of people—a fact well attested—what is to prevent 50,000 human beings, crowded into a square mile, beneath a sultry sun, in July and August, vitiating the air by respiration, and perspiration, and innumerable vegetable substances, from producing a disease, which in *that foul atmosphere*, shall be infectious? Reasoning a priori, I should be led to conclude that Philadelphia and New York may generate plagues, as well as any city in Europe or Africa.—A most terrible plague raged in Rome in the year 80, during the reign of Titus—Another raged in all the provinces of the Empire, in the reign of M. Aurelius in the year 167, which was followed by earthquakes and famine. In the reign of Justinian, about the middle of the sixth century, a plague almost depopulated the earth.

In the year 430, a plague ravaged Britain, and swept away most of the people that had survived the slaughter made by the Picts and Scots. About the year 1348, a most destructive plague desolated the earth. It was said to originate in China and to proceed westward to Constantinople and Egypt; thence along the coast of the ocean to Greece, Italy, France, Britain and Ireland, and thence into Germany, Denmark, &c. Of all these plagues, we have none or very imperfect accounts of their origin. But give me leave to ask, is it within the limits of probability or even possibility, that a disease

should thus be suddenly or speedily conveyed by specific contagion, over whole Empires and the great quarters of the world? And that too in times of savage manners, when vessels were mere boats, and trade either wholly unknown, or limited to a small coasting business. Of the real causes I shall hereafter treat; but I appeal to your own judgment and candor, to decide whether the extensive effects mentioned, can be rationally attributed to the slow operation of communication of the disease from body to body.

The great plague in London, in 1665, is a comparatively recent event; and history has preserved a particular account of it. Is there any proof, let me ask, that the disease was imported? I confess, Sir, I cannot find no better authority for this opinion than surmise. The disease had raged in Holland, in 1663, and parliament prohibited the introduction of goods from that country. Near the close of the next year, 1664, the pestilence appeared in London, and a few people died; but how they became affected by it, nobody knew.

A report was raised that the disease was imported, and the mass of people did then what they do now in America; they took for granted the plague was imported.

The disease made little progress during the winter of 1664, but the summer of 1665 was remarkably sultry and suffocating to persons in health. The plague spread with fury and laid in the dust nearly one hundred thousand persons.

In the account of this fever, we find it expressly stated that when it began to appear in the spring of 1665, it took off first scattered individuals, in different parts of the parish of St. Giles, and no evidence appeared of their having infected each other. You are better acquainted, Sir, with the manner in which it has first appeared in Philadelphia, than I am; but I can assure you, that its first appearance in New York has uniformly corresponded with the above account of it in London.

Russel, who took incredible pains to establish the doctrine of specific contagion, has not been able to produce one particle of proof, that the plague was ever imported into London, further than the positive *opinion* of certain writers,* unsupported by facts. On the other hand many most eminent writers declare there is no evidence of importation. Several facts strongly oppose this idea of foreign origin.

I. Before the Levant company was established, plagues were as frequent and dreadful in London, as afterwards.

II. Before the great fire in London, that city was infested with the plague, nearly one half of the time. The plague of 1603 lasted 8 years—and that of 1636 continued 12 years.

III. Since the great fire in London, not one solitary plague has occurred;

* Among them we find the name of Hodges, who was cotemporary with the plague of 1665.

yet the Levant trade has increased, and some cities in Egypt or the Archipelago are usually infected.

Why, let me ask, has the *effect* ceased, but from the removal of its *cause*? The old city of London was *contrived*, one would think, to breed plagues. The streets were winding and narrow—the houses high and the upper stories jutted over the streets so that they almost met from opposite sides and covered the street below,—add to this, the want of good water. How could such a place escape pestilence? By fire. The city was burnt and rebuilt with wide streets and airy houses. The city was supplied with fresh water, and the modern inhabitants defy the plague.

LETTER VII (November 11)

Several assertions and deductions in your second and third letter to Mr. Wynkoop merit a particular consideration.

In your second letter, you enumerate among the reasons for disbelieving this malignant fever to be of domestic origin, "That such a disease was never known to be generated in Philadelphia, or any other part of the United States before, but had always been introduced when it did occur, by vessels from the West Indies."

Here, Sir, you seem to take for granted a point in controversy which requires proof. The disease has rarely occurred in any part of the United States, to the northward of Norfolk in Virginia, and I am not in possession of the evidence of importation into New York and Philadelphia, when it formerly raged in these cities. I shall therefore not attempt to prove the disease ever to have originated in Philadelphia, or any town to the northward of it, anterior to the year 1793. But, Sir, there is abundant evidence that this disease, and often in its worst forms, has been a regular autumnal complaint in many parts of the lower states.

Messrs. Taylor and Hansford, physicians, in Norfolk, one of them a practitioner of more than twenty years standing, declare, in their letter to me, dated December, 1795, in the most explicit terms, that the same fever which was so fatal, in that year, occurs every year, in scattered cases. Their opinion must be decisive, of the fact, in the sphere of their practice, and therefore I will quote their own words. See my collection, p. 148.

"The fever, we believe, has its origin in the seasons, and exists every year in various grades, beginning generally in August and disappearing about the middle of October. The precise time of its commencement and termination, and its degree of malignancy, depending on contingent causes."—"It differed from the ordinary bilious remittent only in the rapidity with which it passed thro the several stages and in its malignancy—. This last circumstance we are of opinion was occasioned by the long continued and universal heat and

moisture of the atmosphere. The air was evidently impregnated with putrid effluvia, arising from decayed substances of every sort, brot down the creeks and rivers by the floods of rain, and thence into the bason before the town, where, with the filth thrown from the ships and docks, they became for a certain time stationary." Page 150-151. The gentlemen then add the following strong declarations:—"The same fever, with all its malignant and uncontrollable symptoms occurs every year in scattered cases and about the season." They then add, page 152, "We are of the opinion that there is not the most distant reason to believe the fever was imported at all."

The opinion of these gentlemen, drawn from more than twenty years practice and observation, I consider as incontestible authority, and not to be invalidated by any general assertions of medical gentlemen at a distance.

I shall at present offer no other proof of the domestic origin of the plague or worst form of the yellow fever; but only state that my information from the southern states for a number of years corresponds to the testimony of Drs. Taylor and Hansford; and I have the more reason to be strongly impressed with the facts, as not a year passes, in which some of my northern friends who adventure to the southward, for business or residence do not fall victims to this violent bilious fever. It appears to me on general recollection that one half of the northern gentlemen, of my acquaintance who have attempted establishments in the southern states, have died within the first or second year.

True it is, the sudden deaths of a few persons yearly makes no public noise; even if the disease is ever so malignant. While it does not threaten to become epidemic, it creates no alarm. But when some peculiarity of season or contagion spreads the disorder over a whole city, then it becomes an object of notice, and all the world is anxious to know where it originates.

But, Sir, admitting your position that this disease never did take its rise in the United States from domestic causes, prior to the year 1793, is this a decisive evidence that it never can or will originate in our country? It appears to me, that this argument will prove too much, and that London, Marseilles, and even Constantinople, may, by such reasoning, prove that the plague never did or ever can originate in them. In short, the same mode of reasoning will prove that no malignant disease can have any origin on earth, for there must have been a *first time* when every distemper has appeared among men.

The fact then, if true, either proves too much, or proves nothing. But your position ought not to pass without a comparison of facts in different countries.

It appears from Thucydides that the plague in Athens in his time was a new disease in the city. He says "A sickness began first to appear amongst the Athenians, such *as was reported to have raged before this, in other parts*, as about Lemnos and other places. Yet a plague so great as this and so

dreadful a calamity, in human memory could not be paralleled." It is evident from this, that either that disease had never appeared before in Athens, or if it had, that its progress and malignity never had excited attention. Now at the date of this first appearance of the disease, Athens had been built more than eleven hundred years; and during two or three hundred years next preceding the plague, it had been a large and commercial city, constantly importing corn from Egypt, where plagues had been frequent a thousand years before, and how much longer, history does not inform us.

The first plague in Rome, was in the reign of Tarquin the proud; at least it is the first that appears to have been noticed by historians. This was 241 years after that city was built, and when it could muster 130,000 fighting men.

But we have facts of a similar kind of more recent date and of less equivocal authority. The Spanish city of Carthagena, in lat. 10 north on the American coast was settled by the Spaniards in 1533. This is in the very region, destined to be most subject to bilious complaint, yet the *Vomito Prieto*, black vomit, the name given by the Spaniards to the deadly disease which ends with that symptom, was wholly unknown at that place till the year 1729—a period of almost 200 years. My authority for this remarkable fact, is the respectable Don Antonio de Ulloa, a writer of most indubitable authority, and whose account deserves the more credit, as he was at Carthagena himself in 1735, but six years after the *first* appearance of the terrible distemper, which swept away most of the crews of the Guarda Costas, and alarmed the citizens—at which time the event must have been a matter of as much notoriety and common remark, as its appearance at Philadelphia in 1793, is now among the citizens. I shall quote his own words. Voyage to S. America, Vol. 1, p. 44.

"This distemper does not declare itself immediately after the arrival of the European ships in the bay, nor has it been long known here; for what was formerly called Chapetonadas, were only indigestions, which, tho always dangerous in these climates, were with little difficulty, cured by remedies prepared by the women of the country."

"The Vomito Prieto was unknown at Carthagena and all along the coast, till the years 1729 and 1730. In 1729, Don Domingo Justiniani, commodore of the Guarda Costas, lost so considerable a part of his ships companies at Santa Martha, that the survivors were struck with astonishment and horror at the havoc among their comrades. So sudden were the attacks of the disease, that persons walking about one day, were, the next, carried to their graves."

This disease as described by Ulloa, is precisely the yellow fever in its worst form—the symptoms at first hardly distinguishable from those of slighter complaints—but speedily assuming a different complexion—great oppressions at the breast—violent affections of the head—delirium that rendered the patient furious—black vomit, and death the 3d or 4th day in

paroxisms of torture.—It affects strangers only, never is the least contagious to natives—most fatal to the poor and common sailors, but officers and passengers seldom escape.

This, Sir, is a remarkable instance of a new disease introduced into a climate, *without* infection, and from unknown causes. But it is the disease which continues to afflict persons who go thither from northern latitudes.

One fact more. You will probably admit that the Catarrhal affection denominated *influenza*, is a disease, and its first appearance, as an epidemic in the United States, and probably on the continent, was in the year 1789. I have always understood the fact to be admitted by all medical gentlemen. I would then ask, how came that disease in this country? Not by infection, I presume. Where did it originate and how? Shall we not be compelled to resolve such extensive effects into general causes, invisible to our senses? Surely the novelty of a disease then in a particular climate, is not decisive evidence of its introduction by infection.

LETTER VIII (November 13)

In your letter to Mr. Wynkoop, of October 5th, you make large quotations from Dr. Chisholm, to prove the malignant fever of 1793, to be imported to Philadelphia thro the West Indies from Bulama. You afterwards quote several respectable authors to prove the yellow fever, which has appeared formerly in various parts of this country, to be derived from contagion from the West Indies. You specify the following instances:—"Thomas Story, and Joseph Gough, relate that a malignant fever prevailed in Philadelphia in 1699, introduced from the West India Islands"—"Dr. Mitchill ascribes the yellow fever as it appeared in Virginia in 1741 and '47, to specific contagion, and mentions it had been twice imported into Virginia by his Majesty's ships of war." "D. Leamy, in the physical essays of Edinburgh, informs us that the yellow fever which has prevailed at different periods at Charleston, S. Carolina, was always traced to some infected person recently from the West Indies." "In 1741 it was introduced by a chest of wearing apparel, which had belonged to a person who died of it at Barbadoes." Lind.

If we may rely on such respectable authorities, Sir, the yellow fever is an infectious disease, and has been imported into this country at various times, viz. in 1699, 1741 and 1747, and its appearance again in 1762 was ascribed by Dr. Redman to infection from a mariner from the Havanna. These are your authorities.

Now, Sir, it happens that Dr. Chisholm, the very authority you produce to prove the fever of 1793 imported from Boullam, expressly declares, after many years observation, that the yellow fever of the West Indies is *not contagious*—but is an epidemic and sporadic distemper, arising from local

causes, especially heat—that the yellow fever has been known in West Indies ever since the establishment of the colonies; but that the malignant pestilential fever of 1793 was never, as he believes, known there till that year. See his treatise, p. 174, 148.

The question then comes within a narrow compass. Either the first mentioned gentlemen, Mitchell, Leamy, Lind and Redman were mistaken in supposing the yellow fever introduced into this country from the West Indies in 1741, '47 and '62 or the yellow fever, before the year 1793, was pestilential and contagious in the West Indies. There is no alternative.—Those gentlemen were mistaken or Dr. Chisholm is wrong. Either a malignant pestilential fever was known and often *known* in the West Indies, *before* its introduction in Grenada from Boullam, in 1793, or no such disease could have been communicated from the West Indies to this continent in 1741 and 1762.

I leave you to your opinion, Sir, which opinion to espouse; as in either case, some of your authorities fall to the ground, and demonstrate, by their fallacy, that the subject has not been well understood or misrepresented.

That Dr. Chisholm is right in the idea of the *ordinary* yellow fever in the West Indies, is most unquestionable. All the information I can obtain from authors, or from gentlemen who have lived in the West Indies, confirms his opinion, that the yellow fever, or black vomit, as it occurs in sporadic cases, arises wholly from heat, fatigue, night air, imprudent indulgencies, &c. and that it is *not contagious*, or infectious to any material degree. I believe this the more readily, Sir, because in New York we are accustomed to see every autumn a number of sporadic cases, precisely of the same kind, attended with no more infection than the pleurisy or dropsy. And already have such cases almost ceased to excite popular apprehension.

But will you undertake to say that the *pestilential* fever of 1793, in Philadelphia, and of 1795 in New York, was a specifically different disease from the ordinary yellow fever of the West Indies, and from the detached cases of it which occur in New York and probably in Philadelphia, every year? This after all, Sir, must be the resort of the advocates of your doctrine, to maintain and to prove a specific difference in the kinds of fever, and that they arise from causes as distinct as the small pox and the measles. Without this distinction, your doctrine falls to the ground.

If the disease which has alarmed and ravaged many of our towns, since 1792, is *not* the true yellow fever of the West Indies, but a disease specifically distinct from it, and more pestilential, let us give it a different name, and quiet our alarms about the introduction of the yellow fever from those Islands. In this case, we have not to guard our sea ports in summer, from the common West India fever, but from the Boullam fever.

If the distemper which has afflicted our towns, in the form of an epidemic, is the same specific disease, as the ordinary yellow fever of the Islands, differing only in degree of malignity, from local or accidental circumstances, then your theory of contagion is annihilated.

Now it appears, Sir, from the unanimous testimony of the authors before named, that a fever of the type of that which now prevails in Philadelphia, has repeatedly made its appearance in different parts of the United States, *before the year* 1793. The years particularly named are 1699, 1741, 1762. Those authors suppose and assert the distemper to have been imported from the West Indies. One of two conclusions must infallibly follow. Either the fever of the West Indies, does, in certain years, assume a malignant and pestilential form, distinct from the ordinary yellow fever; or the pestilential fever which has appeared in the various parts of our country, cannot be derived from the West Indies, but must be of domestic origin. For the disease which raged in 1741 and 1762, was malignant and fatal, like the disease of a few years past; and if derived from imported infection, it demonstrates the point, that the yellow fever of the West Indies does in fact, assume different degrees of malignity, in different seasons; or that the climate does, in fact, generate two distinct diseases, in different years.

I wish, Sir, to state fairly the facts and premises and to draw from them conclusions strictly logical. If I do not, you readily detect my errors. But it appears to me that a little philosophy and observation will reconcile all the contradictory authorities on this subject. Doctors Taylor and Hansford declare most fully their opinion that the disease in Norfolk, in 1795, which is not denied to be the same as the fever in Philadelphia, in 1793, and in New York, in 1795, is specifically the same disease, as that which appears in scattered cases every year, with aggravated symptoms, derived from local, temporary and accidental circumstances. No men are better qualified to decide this question, than those gentlemen, who have been long in extensive practice in that town.

Not being acquainted with medical science myself, I would not intrude into the province of the faculty: I can only say, that as far as I can collect the opinions of professional men, they seem generally to be decided that the yellow fever of the West Indies, and of our own country, is the same disease, varied only by accidental circumstances, and substantially the same as the plague.

Without recurring to a resemblance of the symptoms, is it not philosophical to reason by analogy on this subject: Is there a greater difference, Sir, between the ordinary yellow fever of the West Indies and its most malignant forms, as it is described by Dr. Chisholm, than between an intermitting and remitting fever, which are confessedly derived from the same cause, acting with different degree of force? Nay, let me ask you as a professional man, whether during the epidemic of 1793 and the present year, the disease has not appeared in different individuals, with degrees of malignity, as distinct as that of the common West India yellow fever, from the usual violence of the present disease?

We read in medical authors that the plague in the Levant exhibits the

same variety in its grades of violence, from the patient who sinks from a healthy state into his grave in 24 hours, to him who runs thro the milder forms of the disease, almost without confinement; yet no man undertakes to say the different patients have not the same disease. If then it is a fact that the common yellow fever is but a milder form of the pestilential disease which has raged in many of our towns, is not the common bilious remittent, a still milder form of the same disease? Should this be admitted, we can find its sources, without recurring to the West Indies or to Africa.

Postscript to Letter VIII

There seems to have been a mistake among writers as to the true orthography of Bulam; some writing it Bulam, others Boullam. It is probable that Dr. Chisholm might have mistaken the spelling from confounding a district of country near the Sierra Leone Settlement, which is called *Boullum*, with the island on which Mr. Paiba and his companions attempted to make a settlement, which is correctly called *Bulama*. Indeed this conjecture is very rational, when it is considered, that Dr. Chisholm supposed this expedition to be a part of the scheme of the Sierra Leone Company. The truth is Sierra Leone is in Lat. 8 degrees and Bulama in lat. 11 deg. The places are at a distance from each other; and the expedition to Bulama was a distinct project, in which the Seirra Leone company had no concern—An inspection of a most correct map of that coast on a large scale, together with Mr. Paiba's information enables me to rectify that mistake.

LETTER IX (November 15)

Thus far I had proceeded, Sir, when I called on Mr. Paiba, the gentleman from whom Dr. Chisholm derived his information, and showing him the doctor's statement, requested him to inform me how far the account given by Dr. Chisholm is correct. This I might well do, without, in the least, impeaching Dr. Chisholm's integrity; as it was very possible he might make mistakes without the most distant intentions.

In this inquiry a medical friend, Dr. Smith, joined me, and made notes of the facts related. Mr. Paiba was one of the twelve councilors or directors of the expedition to Boullam, and well acquainted with every material transaction. In addition, his testimony as to most of the particulars, is confirmed by the recollection of Mrs. Paiba, who attended her husband to Boullam and to Grenada, and who is a very worthy and intelligent lady.

From the minute information of both these persons, I am enabled to correct a great number of material mistakes in Dr. Chisholm's account of that expedition, and the introduction of the fever into Grenada.

Dr. Chisholm states, page 83, that the ships employed in that enterprize

were both chartered by the Sierra Leone company. This is an error; the Sierra Leone company had no concern in the enterprize.

In page 85 and 86, he mentions that the water at Boullam is brackish and unwholesome. Mr. Paiba informs us, the water is good.

In page 85, the Doctor says, during nine months that the Hankey lay there, there was no accommodation erected on shore, but the people were the whole time obliged to live on ship board. This is a mistake. The Calypso returned to England without erecting any buildings on shore; but the Hankey's people cleared several acres of land, and built a block house, with four gates, and planted artillery to defend them; and when she left the island, one of the council and 32 of the company remained there with all the provisions, determined to persevere in the attempt. The sickness broke out on board their ships, and increased on shore.

In page 84, the Doctor represents Boullam as surrounded by the ocean and enjoying the advantages of sea breezes. The truth is, that island is one of 15 or 20 which lie in a large bay opposite the mouth of Rio Grande, forming an archipelago. Boullam itself is one of the innermost islands in the bay, formed by the mouth of the river, and about 50 miles from the entrance, from the sea—of course entirely sheltered from direct sea breezes, by the outer islands. It is in 11° 30′ N. latitude, and of course feels a sultry sun and violent rains at a certain season.

In page 86, Dr. Chisholm makes no doubt that neglecting to sweeten the ship and ventilate her, and not destroying the clothes of the deceased, was the cause of her retaining the infection till she arrived at Grenada. Now it happens that Mr. and Mrs. Paiba both declare that before the ship arrived at St. Jago, the utmost pains were taken to cleanse her. The berths of the deceased were all cleaned, the bedding destroyed, and the hold was repeatedly fumigated &c.

In page 87, he states that of four men furnished by capt. Dodd at St. Jago, *two* of them sickened on the third day after leaving that place, and died. This is a mistake. Of the four men, one of them only died, and he was indisposed before he came on board the Hankey.

In page 88, Dr. Chisholm says the Hankey arrived at Grenada on the 19th of February. This is a mistake. She did not arrive till late in March, about the 18th or 19th, for the ship arrived at St. Jago between Christmas and New Year, Christmas being kept by Mr. Paiba and family on the passage from Boullam to St. Jago—and the ship remained there *two months*—that is, till the last of February. She was then 17 or 18 days on her passage to Grenada. But Dr. Chisholm himself represents the fever as making its appearance early in March, and of course, *before the arrival of the Hankey.*

Of captain Remington's case, Mr. Paiba gives an account very different from that of Dr. Chisholm. Capt. Remington did not go on board at the time stated by Dr. Chisholm, as Mr. Paiba believes; but at any rate, he was all day and night coming from Grensville bay—had been wet to his skin—slept

in his clothes, and returned to his own ship in an open boat—hardships enough to kill any man in that climate without infection.

In a number of other particulars, Dr. Chisholm's account is inaccurate; and it proves that he had either been careless in taking minutes of facts, or had a preconceived system which this compilation was intended to maintain. These mis-statements alone go very far to shake my faith in his whole account of the Grenada fever—And my incredulity is increased by observing multitudes of even sensible men in this country, who attempt, in the same careless way to trace our autumnal fevers to some single ship.

A brief state of facts relative to the Boullam fever is as follows. Two ships, the Calypso and the Hankey, with a Sloop, sailed from England for Boullam, with about 300 persons including the crews of the ships destined to make a settlement on that Island, for the purpose of cultivating cotton for the Manchester market. They put into Teneriffe, where the crew of the Calypso, being infected with the small pox, were not suffered to go on shore. This ship therefore proceeded on her voyage and arrived at Boullam two weeks before the Hankey. The latter ship remained at Teneriffe a few days and received refreshments.

When the Hankey arrived at Boullam, the crew of the Calypso had already become sickly, and the fever not long after broke out on board the Hankey. But it is agreed on all hands, that all the people arrived in health; and that the fever originated on board the ships.

Some of the Calypso's people had landed on the Island of Boullam, before the Hankey arrived with the Governor and principal part of the council, and very imprudently too, before any offer of purchase had been made to the King on the neighboring shore who claimed the Island. The natives thot they were to be robbed of their land, and sent a number of men in the night, who killed three men and took five women prisoners—who were however ransomed by the Company. This circumstance, with the sickness, disgusted many of the company and the Calypso returned to England, with more than half of her original complement.

The Hankey remained, and as before remarked, her people made a slight attempt at clearing the land, and building houses for accommodation. But the disease was aggravated on shore, by the heat of the weather and the fatigue of the people.

From May to December the remaining part of the company was much reduced, and one of the council and 32 men alone resolved to prosecute their scheme. Mr. Paiba, with Captain Coxe in the Hankey, left Boullam with 19 persons, eight of whom died before reaching St. Jago.—It should however be noticed that the ship left Boullam November 22, but getting aground, and being weak-manned, she was not got off for a long time and did not clear the bay, till near Christmas, nor arrive at St. Jago, till about the last of December.

Before and after this the ship was well cleaned and fumigated. The people on board were not well, mostly indisposed with intermittents, but all recovered before leaving St. Jago.

Many people of Port Praya, at St. Jago, and the governor were frequently on board the Hankey, while she lay there, which was two months, and Mr. Paiba never heard that any person was affected, or that any evil result from the communication. Mr. Paiba and Lady were frequently on board the Charon, Capt. Dodd's ship, and no ill consequence resulted from it, nor did Mr. Paiba ever learn that the crews of the Charon and Scorpion were affected by infection from the Hankey, nor is it possible this could take place, as not a seaman was permitted to go on board the Hankey—The Captain's barge which frequently waited on Mr. Paiba and Lady to and from the Ship, had strict orders not to let a man go on board—and it is beyond all belief that men along side of a ship which is constantly exposed to air and water could take a disease by infection from the outside. Besides, the Hankey lay two months in the harbor and had infection been communicated, it must have made its ravages among the crews of Capt. Dodd's ships, long before Mr. Paiba sailed from Grenada.

If then part of the crews of the Charon and Scorpion actually sickened and died afterwards, it must have been from original diseases among themselves; for they were fresh men from a northern latitude—Capt. Dodd not having arrived at St. Jago, till the Hankey had been there near a month.

On the passage of the Hankey from St. Jago to Grenada, one man, who had been some time indisposed, died.

The Hankey stopped at Barbadoes and at St. Vincent's, on her way to Grenada—at both of which Islands, there was a free intercourse between the ship and the islands, the crew going on shore, and the inhabitants going on board; and there was never a suggestion of any infection conveyed from the ship at those places. Let any candid man therefore say whether he believes that this ship introduced a pestilence into Grenada.

Still more incredulous shall we be, if we attend to the situation of the port of St. George's where the sickness broke out and raged. The harbor is made by a bay, on one side of which is the Carenage or creek which runs some distance into the land, forming a marsh or muddy inlet. The winds that come over this place are disagreeable, and here began the sickness.

Mr. Paiba declares that in conversation with Dr. Chisholm on the subject of the distemper, the Doctor never pretended that it took its origin from the Hankey. On the other hand, he told Mr. Paiba, that a few years before a similar fever had prevailed at St. George's. Indeed Dr. Chisholm himself relates that some people on the island were of opinion the disease was *not* imported as it *appeared in various distant places at the same time.* This concession of the Doctor is the best evidence he could have produced against his own opinions.

Luckily we have a clue to unravel the whole mystery of the Doctor's system. In page 83, he mentions that these ships were chartered by the Sierra Leone Company, and that the people had a "fanatic enthusiasm for the abolition of the slave trade." The latter idea has some foundation, tho the Sierra Leone Company had no concern in the expedition.

It seems then that the people in the West Indies were alarmed at the attempts of these companies to abolish the slave trade and raise cotton, which might injure the West Indies; and in order to defeat them, represented the African coast as infested with pestilence. They of course took unwearied pains, to make it believed in England that the climate was fatal to life, and actually succeeded in their malicious designs. A full account of their misrepresentations is found in an essay of Colonization, published in 1794; of which a particular statement will come before the public in another form and by another hand.

I will only observe in general that every species of falsehood and misrepresentation, was resorted to by the enemies of the Boullam enterprize to discourage the prosecution of the settlement, which might result in putting a stop to the nefarious slave trade, and in establishing the culture of cotton by free hands. They raised such alarm about the Hankey, that they succeeded in obliging her to perform quarantine in England—and they raised a report that every person who came near her died, and that she had been scuttled and sunk, with her whole cargo of sugar.—This was not true; but it shows what infamous means were employed to throw an odium on the expedition to Africa, and how little we can rely on the account given us of the origin of the fever in Grenada.

As far as respects the discouragement of the settlement, and to the continuance of the slave business, every man will regret the success of this maliciousness, and execrate the men who were its authors and instruments of propagation.

But when we reflect that a book has been written for the purpose of vindicating a false theory of specific contagion in the yellow fever, a theory that involves the health and happiness of men in all parts of the earth, where is the epithet that will express the baseness of the undertaking, or describe the melancholy effects of the principle? Surely, Sir, men have not thus a right to trifle with truth, and with social confidence and happiness. I conceive, Sir, this attempt to deceive mankind and to propagate an error for the sake of the local interests of the West Indies, an evil of more consequence to society, than a dozen plagues.

LETTER X (November 17)

In your letter of the 10th of October, to Mr. Wynkoop, you assert, that "in 1795 when the season was *cool* and wet, some of the very *learned* physicians of New York ascribed the fever to the mud of the docks, and in the face of glaring facts, denied that it was contagious, tho it was proved by unquestionable facts that it was introduced from Port-au-Prince by the brig Zephyr." You then quote, in proof, a letter from the New York board of health, which, I find, is dated Sept. 8, 1795.

I will omit, Sir, all remarks on the epithets "*very learned*," applied to the physicians in this city, on which you have laid a great emphasis.

But I am sorry to say that the foregoing representation is not founded on facts. The weather of the summer of 1795 was on an average unusually warm, and Mr. Professor Kemp, of Columbia College, has testified that the mercury, by Farenheit, was, on an average that summer, *three* degrees higher than it had been during the ten preceding years. See Bailey on the fever, page 54. A few cool days occurred about the beginning of Sept. when the board of health wrote their letter, and the fever almost disappeared, tho it afterwards spread with increased mortality. Three degrees by Farenheit, for a day or two, is a thing of no account; but when continued for weeks, is a most influential circumstance.

True it is the season was wet, but this was an unfavorable circumstance, because united with excessive heat, and a close unelastic air. For weeks together the air was so moist that no electrical fluid could be collected; and the progress of putrefaction was, beyond all example, rapid. I myself purchased sound potatoes in market, which, in 36 hours, were a mass of putrefaction; and the heads of cabbages rotted and fell from their stalks in our gardens.

Your assertion that unquestionable facts prove the fever in New York to have been taken from the Zephyr, is a proof that you have never attended to the evidence on that question. The board of health state only that Dr. Treat sickened and died soon after visiting the brig Zephyr, but not that he *took the fever by infection.* On that question they gave no opinion.

Admitting, however, all you wish, that Dr. Treat caught the fever by infection, it does not help your cause in the least; for no person who attended or visited him ever took the fever from him, nor did the fever, thro the whole season, prevail in the neighborhood of his house. On this point, therefore, you derive no aid from facts.

Equally unavailing is the attempt to prove infection communicated from the Zephyr to the ship William; as it has been demonstrated by indubitable testimonies, that the Zephyr was never within half a mile of the William. Had you ever read the publications of Dr. Bailey and Dr. E. Smith on the fever in this city, you could not have fallen into such material errors.

It happens further that a person died in the poor-house in this city, of the very same disease, about fourteen days before the Zephyr arrived in port; and I was informed, at the time, by the late health officer, W. P. Smith, that he had been called to visit a patient, a blacksmith, about the same length of time before Dr. Treat's death, who was affected with the same disease. No person ever pretended that either of these people had caught the fever by infection.

Your representation that the *learned* physicians of New York ascribed the fever to the mud in the docks, is wholly unfounded. No expression in their communication will warrant the assertion. They say nothing in their letter to Gov. Jay, of Sept. 5, 1795, of dock mud. The board of health speak of the place where the disease first prevailed, as being low, and abounding with offals and filth, and crouded with poor people. This representation was just and I cannot resist the evidence that these causes contributed to the fever, for that part of the city is cleansed, and has not since been troubled with the fever.

The idea of the New York physicians I have understood to be that the mud of the docks, which is below high water mark, and of course often covered with water, is not prejudicial to health. And this opinion of theirs is strongly confirmed by the fact, that the sweepings of the streets, which are excellent manure, after being some time under water, lose the properties of manure—a fact which proves the vegetable acids or nutriment of plants, which are known, in a decompounded state, to be injurious to health, are separated from the earth and filth and absorbed by salt water.—leaving the mud a mere caput mortuum.

It is the opinion of our medical gentlemen, that the filthy substances which vitiate the air, are those which are lodged on the surface of the earth, mixed with fresh water, or exposed to the action of heat. The docks therefore are harmless, except such parts as may happen to be above the overflowings of the tide.—So much I have thot proper to say, in answer to your statement respecting New York.

But, Sir, the physicians stated in their letter of the 5th that the disease of 1795 appeared to them of the same kind as one which often occurs in the summer and autumn. And, most unfortunately for your theory of its introduction from Boullam in 1793, the same fever, with all its "malignancy and uncontrollable symptoms," appeared in Water and Front-streets in this city, one or two years before it did in Philadelphia in 1793, and before the expedition to Africa, from which you have derived its origin. Yes, Sir, the fact defies your theory and your proofs. Long before that pestilential fever appeared in Grenada, it broke out in the low streets of this city, with an alarming mortality, and speedily carried to their graves about 200 of the citizens, among whom was the worthy and respectable Gen. Malcolm. And, Sir, I will thank you to exercise your talents in discovering the origin of that

disease; because, according to *your* theory, and that of Dr. Chisholm, it could not come from the W. Indies, where the people have never been troubled with a worse disease than the common yellow fever, till 1793; and it is known that the Boullam expedition was not set on foot till one or two years after.

In the year 1793, I know of no facts on this subject in N. York that deserves notice, except that several persons died here with the disease contracted in Philadelphia, but not the least symptoms of contagion as far as I can learn, was ever discovered.

In 1794, we had a fine season—our atmosphere was often purified as well as our city, with cool rains and northerly winds. Yet we had from 20 to 30 cases of the yellow fever, ending in black vomit—and in one instance, I understand, there was an appearance of infection. This was in the case of a foreigner and his lady who had recently arrived.

In 1795, the summer was probably the most unfavorable to health, that we ever knew, and the disease appeared in this city, spreading along the East River, and into the low grounds on the north east. At the same time, a fever and dysentery appeared in many of our interior towns, with a mortality equal to that of the yellow fever in New York. In some parts of Dutchess and Columbia counties, the fever was *more fatal* than in this city; and I have the opinion of a medicinal friend, who attended many of the patients, that in one instance, he attributed the fever to infection; so that if *vegetable* effluvia produced the fever originally, it appears that a person breathing the air of the chamber in which the diseased lay, *caught* a fever of the same type—a fact which, if well founded, overthrows your first principles.

In the western counties of this state, a fever often prevails in the vicinity of some of the lakes, which is as fatal as the fever of 1793 was in Philadelphia; and even more so. It was so in 1795, that most unhealthy summer.—This Lake fever, you may say, is a different kind of disease, from that which prevails in our cities. If this is true, still the fact recurs, that vegetable putrefaction, in marshy grounds, may produce epidemic fever *as mortal as the plague*. Perhaps the absence of *one* cause of our city fever, human effluvia, may vary the symptoms in a degree, and render the disease *less infectious*. But the epidemic about the lakes, for a few seasons past, tho it may not demonstrate that our climate produces the yellow fever of the West Indies, or the plague of Egypt, will, at any rate, demonstrate that our climate does often generate diseases much like them and as mortal as either one or the other.

These facts, Sir, speak their own language—the language of truth, which can neither be parried by sophistry, nor resisted by argument. Ingenious men and honest men may, from early prejudices or from a partial examination of the question, believe and attempt to make others believe, that our country is more favored by heaven than other regions of the earth under the same parallels of latitude. They may see mortal diseases break out on our wharves

and ascribe it to imported infection; and it may be difficult to disprove the fact when vessels are daily arriving from the torrid zone, and the exciting cause of the disease is invisible.

But when fevers of a bilious type break out, in remote interior situations, three hundred miles from a seaport, where no infection from abroad can possibly be the source of the diseases; when these fevers become epidemic, and sweep away multitudes of men, women and children, in a few weeks, the philosopher and candid citizen will desert the indefensible ground of importation, and will acknowledge, that *our climate* has obtained no decree of heaven exempting it from the operation of the general laws of nature. He will be compelled to admit that the vegetable and animal substances of *our climate* are composed of the same elements as those substances in all other countries —that when they dissolve in *our country*, they extricate the same deleterious acids, as in Africa or Asia, and that even *our atmosphere*, is capable of being as thoroughly vitiated and unfitted for animal respiration as the air of Egypt or Alleppo.

We may cavil about the hairbreadth distinctions in diseases, till we are grey, and while our citizens are dying with epidemics, we may wrangle about names; but we still must all agree that the air we breath, if vitiated, will not support healthy life; and whether we call a disease the plague, yellow fever or a bilious remittent, and whether it is a child of our own, or of foreign birth, we know that *an impure air is the nurse of the infant*, without which it would perish in the cradle.—This knowledge illustrates our true interest and is decisive of our duty.

LETTER XI (November 21)

In deciding the important question of the domestic or foreign origin of the plague, facts are the best arguments.

In the year 1795, the pestilential fever in N. York broke out a little northward of Peck's slip. I examined the ground in that quarter very carefully, and am confident the filth accumulated there was a cause adequate to the production of disease. Timber for the front of a wharf had been laid, and the wharf not filled. The tide flowed in to some part of it, and the whole was a reservoir for all species of offals and rubbish. Some stores about the same place were built on piles and beneath them had been collected every thing nauseous.

The sufferings of our citizens that year led to a correction of these nuisances; and no fever has prevailed there since.

But it is remarkable, that in 1795, the fever progressed only to the coffee-house in Wall Street.—At least it occurred only in a few scattering cases below that street. To the southward, the city was almost exempt from disease, even on the low grounds along the water. But during the very

autumn subsequent, the foundation of a new wharf was laid at Whitehall street and from that towards the Exchange slip. The timber was laid, but the wharf was only partly filled that season; and people did there what they always had been accustomed to do; they threw every species of filth, dead dogs, cats and horses, &c. into the cavity.

In the month of April succeeding, (1796) Dr. Bailey examined the situation of that part of the city, and made a report to the corporation; assuring them, that if the nuisance was not corrected, that part of the town would be infected with malignant diseases, in the course of the summer. And so the event proved. The yellow fever, the plague, broke out as early as June and alarmed the city, but still more the country, and stopped a great portion of the ordinary intercourse. A series of rains and northerly winds in July checked the progress of the disease; but warm weather again excited the pestilence, and about one hundred and fifty lives fell a sacrifice.

And it seems that nature, true to its laws, limited the disease to the region of infection; for it was confined, in its ravages, to a distance of 2 or 3 hundred yards. Yet the alarm had gone forth, and altho nineteen twentieths of the city was as free from sickness as ever, yet a great part of the business of the season was lost.

That nuisance and others near it are now removed, and this season, no pestilence has disturbed our tranquility.

Facts of this sort are not to be reasoned away by airy speculations. They stand on the records of every citizen's knowlege, and irresistibly impel conviction.

During the summer just past, the disease has appeared in a few sporadic cases, with all its "uncontrollable symptoms" and there is no possibility of tracing one case, as I am well informed, to any infection from abroad. On the other hand, the city commissioners assure me, that they can, in all the cases, except two, trace the diseases directly to some mass of filth near the stores or dwellings of the diseased. In one case, the fact is so striking as to have convinced persons who before disbelieved the domestic origin of the fever. This instance occurred opposite the fish market. In the rear of the houses fronting that market and on a part of the square that was burnt last December, there was an unoccupied space near the water, on to which every person took the liberty to throw filth, dead cats, &c. The nuisance was not corrected in season, and *seven* persons, in the buildings next adjoining, fell a sacrifice to the fever. These persons lived to the leeward of the place, as the wind usually blows; and in the same direction, at a greater distance, several others sickened, but recovered. The line of infection from this source was nearly as direct as a mower lays a swarth of grass in the field. The fact was so convincing, that scepticism itself has yielded to the evidence of the generation of the disease in that spot.—I could specify other instances similar to that; but I think better to omit them, as the commissioners will state some of them

in their report, and others will be carefully described in the next number of the Medical Repository.

Sporadic cases of fever similar to that which has afflicted our sea ports, have often occurred in various parts of the country.

In 1792 and 3 Dr. Reynolds informs that an "epidemic fever attended with the same symptoms as that in Philadelphia," prevailed in the town of Galaway, above Albany in this state, and it proved fatal to many persons. One case he describes was marked with most malignant appearances. On the borders of a marsh, a horse died and putrified. A young woman who frequently passed that way was seized with violent pains in her head and sickness at the stomach. She was bled on the second day, but the fever increased, she became delirious and died on the fourth day, with her fingers and toes covered with blisters, or spots the edges of which were inflamed.—From such facts, Sir, I leave you to judge whether the worst forms of typhus fever may or may not have their origin in our climate.

Of what species is the fever which has been so destructive to people in Norfolk and Baltimore, this summer past: Is it a distinct and specifically different disease, from that which has scourged Philadelphia? This is a material point. If the disease is distinct, then our controversy is at an end; for we are discussing different points. If the yellow fever which has appeared in all parts of the United States, since the year 1791, is every where the same disease modified only by season, place and constitution, then it must have, in every case, proceeded from the West Indies, or must in some places, be of domestic origin. Now it is proved beyond contradiction, that the fever has repeatedly appeared in New York, Baltimore and Norfolk, (and once in Boston) without infection. It may and does therefore have its origin in our climate.

If the fever in Philadelphia is a *different* disease; still we shall find difficulty in tracing it to the West Indies, for the West India fever attacks strangers only in that climate and is not infectious nor contagious. This is an agreed point by the best writers on that disease. But the disease in Philadelphia attacks citizens as well as persons not accustomed to the climate, and is stated to be highly infectious.—If the disease is a different kind in different places and all imported, then we come to the result once before stated that the West Indies produce two different sorts of yellow fever.

It belongs to the medical profession to decide the question of the identity of the disease; and on that subject I shall give not positive opinion. I am inclined to the belief that all the forms of this malignant distemper are but varieties of the same specific disease, because this opinion appears to be supported both by facts and philosophy.

The dysentery exhibits similar varieties. Sometimes it is a slight complaint.—at other times, it is as fatal as the yellow fever. The small pox and

the measles appear under similar varieties. The fever at Bulama, from which, it is said, all the plagues of the West Indies and the United States, have proceeded, exhibited all the grades of bilious fever from an intermittent to the plague. For this assertion I have the authority of Mr. and Mrs. Paiba, who saw it in all its varieties. In its milder form, it yielded to large doses of the bark—Mrs. Paiba herself broke the fever on its first onset, in this manner—and this by the way does not support the idea that the Bulama fever was the origin of the fever in Philadelphia; for the disease in this country, has not admitted the use of the bark, scarcely in any possible shape, unless in late stages of convalescence.

In its more violent attacks at Bulama, it soon destroyed the powers of the mind and the patient became an idiot.

The plague in the Levant exhibits similar varieties. Russel arranges the cases in six classes. The most fatal cases ended in death in 24 hours: the most numerous of them were prolonged to the fifth or seventh day; and many persons were so slightly affected, it was difficult to decide whether they had the plague or not. When the plague in Aleppo exhibits such various grades of malignancy, why should we be stumbled at varieties in the bilious fever in our country?

One fact more, Sir. The disease of Philadelphia in 1793 and '97, has exhibited very decided evidence of contagion, while the fever of New York in 1795 and of Baltimore and Norfolk, has as decidedly *not* been propagated by contagion. Few instances indeed in this city can be traced to infection from the sick. These circumstances have led very judicious men to question whether the disease in Philadelphia is not of a different species.

To this conjecture however there are substantial objections. In 1793, several persons came from Philadelphia and died in this city.—They unquestionably had the same disease as their fellow citizens: yet no infection followed the malady, among the attendants in this city. The same fact has occurred the summer past, in a number of cases: especially in a vessel from Philadelphia. The sick were carried to the hospital; nay more, persons afflicted with West India Fever have been carried from their vessels to the same lazaretto and not one person nurse, physician or boatman has been in the least affected by contagion. How happens this? Will you ascribe this effect to the salubrity of the air in and about New York? But if your fever has specific contagion it could not lose the property, by being transported only one hundred miles.

A similar fact occurred in 1794 from New Haven. Mr. Austin, one of the first who received the infection at New Haven, came to New York, sickened and died. No one knew at the time he was seized that he was infected with the yellow fever, and his friends visited and attended on him without apprehension.

LETTER XII (November 22)

If we attend to certain phenomena which usually attend pestilential diseases, which are denominated the plague, yellow fever and black vomit, and which appear to be the same in all countries we shall see a manifest difference between this class of diseases and those which are propagated solely by specific contagion; and perhaps find strong reasons to believe that all bilious complaints from an intermittent to the plague are of the same species. One of the most general and prominent circumstances is its disposition to attack strangers or persons newly arrived in a particular climate or atmosphere. Another circumstance is its commencement usually along the borders of the sea, or rivers. A third is its augmented violence in low, narrow streets, and in small filthy houses.

The real Levant plague however seems to make very little difference between strangers and natives. The fever in the United States has, in its worst forms, discriminated between natives, and West Indians; the latter having been less subject to it, than the former. And this has been visible in Philadelphia, where the disease has assumed its most deleterious symptoms. Still the fact of a difference in the predisposition of natives and strangers to this disease, is more or less a subject of observation in all countries.

In the plague at the siege of Syracuse, mentioned in a former letter, the historian Livy takes notice of the greater mortality among the Carthaginians, than among Romans. The Carthaginian troops were new recruits, and were exposed to all the inconveniences of a hot sun and an unfavorable position in the country—they died almost to a man. The Roman troops had been long before the place or in the country; they were used to the air and water; their encampment was in a more healthy position, and they suffered less.

Dr. Chisholm, in his account of the Grenada fever, had given an exact scale, marking the different grades of mortality. Among the robust sailors and soldiers, who had recently arrived in that climate, the deaths were *one* to *three* of the sick; among the soldiers who had been three years in the country, *one* to *twelve*—Among the blacks and mulattoes, *one* to *eighty-three*.

Don Ulloa takes notice of the same fact, in the black vomit on the Spanish main. And this is uniformly the case—the natives see, without sickness or alarm among themselves, strangers attacked and carried off in multitudes, and in a few hours. The same is observable in all the West India Islands; and uniformly true also in the southern states of America.

In New York, the persons who fell victims to the fever of 1795, were three fifths of them recently from Europe, and as the persons of that description did not compose a twentieth probably of the citizens, their predisposition or liability to the disease, compared with that of old citizens, may be estimated as thirty to one.—These facts are notorious.

The usual appearance of this distemper on the borders of the ocean or of

the rivers is another remarkable circumstance, and has given rise in Egypt to an opinion that it is always imported; and the fact is also alledged, in support of the same opinion in this country.

But it appears to me, Sir, that a humid state of the atmosphere, in a hot climate or season, is among the *causes* of the disease. Its appearance in Egypt in winter only, when the air is humid, is a fact confirmatory of this supposition; and I find this to be the opinion of Dr. Mead.

But tho the plague appears *generally* on the sea shore first, this is not always the case. The great plague in the time of Thucydides is said to have originated in Ethiopia; and the most fatal pestilence that Egypt has experienced this century, that of 1736, was universally acknowledged by the Egyptians, to have had its origin in upper Egypt. Russel. P. 3. These facts show how superficially those authors, mentioned in a former letter, have attended to the history and progress of the disease, when they assert that it never originates in Egypt, and that it is always imported. While therefore we all acknowlege that malignant bilious fevers are most usual in the vicinity of water, we can prove that the worst forms of those diseases have originated several hundred miles from the sea.

But the fact of most consequence is the increased violence and mortality of these malignant diseases, in narrow streets and low dirty confined houses. On this fact, which is common to every species and every grade of bilious complaints the legislator and the magistrate will found their regulations for preventing these diseases or mitigating their effects.

Most writers from Thucydides to this day, who have described malignant fever, have mentioned this important circumstance, that they are most frequent, violent and fatal among the poor, in crouded houses and damp filthy places.

Russel on the plague at Aleppo page 61, remarks, that in the villages and in the Keisarias, or clusters of huts in the cities, inhabited by the poor, the plagues made the greatest havoc, owing to the huts being crouded, and small, and having few or no windows. People in middling life, suffered less; and the higher classes, who have large, open, airy houses, pass their time in the city during the plague with very little hazard.

The same fact is observable in the United States—it has been noticed in Norfolk and Baltimore; in Philadelphia and New York. In New Haven, the fever was almost limited to the shore or the borders of the creek; at least its pestilential effects were there principally felt. In Newburyport and Providence, the same fact has been observed.

But New York, of which I am best qualified to speak with positiveness, exhibits the different operations of this disease, in low and filthy, and high and airy situations, in a manner to force conviction. Three times since the year 1790, has the yellow fever appeared in this city as an epidemic along the shore on the eastern side of the city; yet in no instance have its pestilential effects been prevalent to the westward of Pearl Street, except in a

few low streets in or adjoining the swamps on the North of the city.—It is a fact most prominent and remarkable, that in the epidemic of 1795, almost every family in Water Street down to the Coffee house, suffered more or less by the fever; yet in Pearl Street, a distance of from 50-70 yards only, almost every family escaped.—A few cases of the fever occurred in Pearl Street, but it was not an epidemic.

And whenever the fever has broke out with violence near the water, and persons, whose counting houses were on the wharves or who had business there, have been seized with the fever, and have been very ill or died in their mansion houses, on the high grounds westward of Pearl street, the fever has not exhibited infection; nor ever spread from such cases as a source of pestilence. If a single exception to this remark has ever occurred, which is possible, the observation, as a general one, is strictly just.

It has been every where observed, that the fever, carried into the country, has not often been communicated, but has usually died with the person carrying it. A few exceptions, it is said have occurred this summer past; but in general the fact is so. This is equally true with the fever when taken on the wharves in this city, and carried back upon the elevated situations, on the middle of the Island. Examples of this fact have been so numerous and so obvious, as to demonstrate, beyond the power of contradiction, that the infection attending this fever is not a *specific contagion*—but is altogether an accidental circumstance. It proves in short that the disease proceeds from a vitiated state of air, and that its power of producing itself, which is infection or contagion, consists solely in its power of vitiating the air. In an open airy room and a pure atmosphere, the effluvia from a single diseased person, where cleanliness is preserved, will seldom vitiate the air in a sufficient degree to affect the nurses or other attendants, with the same disease. But in narrow streets, unventilated rooms, in filthy places, or where many persons are crouded together, the air becomes so highly charged with noxious effluvia, as to infect well persons who are obliged to breathe that vitiated air for any length of time. Whatever may be the case in other places, I am confident I speak the opinion of a great part of medical gentlemen in this city, when I declare that the yellow fever exhibits no other contagion than what is above described. And it is on this fact, as a foundation, that all our police regulations are to be constructed.

Still further; to prove that the yellow fever in this country and in the West-Indies, are the same disease, and depend for existence on the same causes, I offer the following facts.

Rock Fort in Jamaica is situated on the lower grounds, and has in its neighborhood marshes which constantly emit noxious exhalations, and the troops stationed there are always sickly. In the year 1792 a detachment of the first regiment or royals, to the number of 40 men was stationed at that place of which more than 30 died. The remainder returned to head-quarters at

Up Park Camp, on high ground, remote from the marsh and recovered. But they did not infect the soldiers in that camp. On account of the certainty of sickness at Rock Fort, that place is never garrisoned in time of peace.

Here we observe the disease even in the torrid zone, exhibits the same characteristic phenomena, as in our climate. It appears with all its fatal malignancy in low marshy grounds and disappears, as an epidemic, on high and well ventilated lands. See Dr. Bailey's collection page 66 to 74, for other examples.

Mr. Paiba informs me that the sloop which accompanied the expedition to Bulama, as a tender, was used as a kind of hospital, and the sick, when sent aboard of her, being free from foul air, would speedily recover; but when they returned to the Hankey, to the focus of vitiated air, they were again taken ill. This circumstance proves the same fact, that foul air, independent of climate, is the source of pestilential diseases; and that, even at Bulama, the disease lost its force and power of *infection*, when removed into uncontaminated air.

It was a common thing in the New York fever of 1795, for the sick poor, to be removed to Belvue and they were conveyed in boats on the river, the distance being about two miles. Such was the effect of fresh air, on the sick, that persons, who could not bear their own weight, when carried from filthy dwellings in damp and narrow streets, were so revived by being exposed to free circulation on their passage, that they would often walk from the boat to the Hospital, a distance of forty or fifty yards on rising ground.

Would the same effect be experienced in any situation by persons affected with a disease of specific contagion? I submit the question to your judgment and experience.

LETTER XIII (November 24)

I think, Sir, it has been clearly proved in the foregoing letters, that malignant fevers of a bilious kind, whether called plague, yellow fever or typhus, do not possess the power of a *specific* contagion; that is, a power of communicating the same disease from a sick to a healthy person, independent of season and local circumstances—Indeed that the infection they exhibit, is solely an augmentation of a deleterious quality of the air, which produced the original disease.

The evidence leading to these conclusions is to me irresistible. It appears to me from all I can read and observe that your observation, in a paper dated Oct. 9 which you published in the Gazette of the United States, and in which you say, "the plague is a higher grade of the yellow fever, as the yellow fever is of the typhus or contagious fever," is well founded; and that the danger or probability of infection from the disease is in a direct proportion to these grades of violence in the disease united with the local situation of the patient. If the patient is in a place where foul air is generated

in great abundance, and the means of ventilation and cleanliness do not exist, the effluvia from his body will prodigiously augment the poisonous quality of the air, and expose attendants to the same disease. If the patient is removed to a place where no foul air is generated from other causes than himself, and the room is well aired and kept clean, the chances are twenty, perhaps a hundred to one that the air will not be corrupted in a degree sufficient to produce the same disease in a healthy attendant.

On what other principle, Sir, shall we account for the fact related by Russel, in his history of the plague at Aleppo, that on the first appearance of the distemper, it was *most fatal* to those affected, but less *contagious* than after it had become general. In page 78 he says, "In the plague of 1760, the disease was always regarded as dangerous, but the degree of danger was very different at different stages of the season. At the beginning, few or none of the infected escaped death; as the season advanced, a certain proportion of the sick recovered; and in its state and decline, the proportion of such as recovered was greater than ever." In page 296, he remarks, "In the early period of a pestilence, when the *most fatal forms of the disease are prevalent, the assistants often escape unhurt.*"

In page 297, "It is probable, however, that the *danger* incurred by approach to the sick is *less* in the *first* state than it becomes afterwards, tho the disease itself may be no less contagious." The author then assigns reasons for this encreased danger—viz. the augmentation of pestiferous effluvia in the rooms, bedding, cloths, &c. The same facts were noticed in the plague in Marseilles.

I think these facts important. They correspond with what has appeared at least in some towns in the United States. At Providence, the summer past, it was observed that on the first appearance of the disease, every person who was attacked fell a victim—In the later stages of the distemper, it assumed a milder form, and many recovered.

The facts totally overthrow the doctrine of *specific* contagion. They demonstrate that the infection depends on circumstances. The frequent escape of nurses, on its first appearance, proves that the infection does not proceed so much from a communication with a diseased person, as from a general state of the air, which, in the progress of the disease, becomes highly charged with noxious effluvia. At the same time, while this general noxious state of the air affects a greater number of persons, yet the persons breathing it for some time, become accustomed to it and lose their sensibility to its action, so that the attacks of the disease are *more slight*.

In short the facts above cited correspond in principle with what we observe every year, and what Dr. Chisholm has reduced to a scale—That the violence of this disease, instead of depending on specific contagion, depends almost solely on the sensibility of bodies to its action, and this sensibility is greatly affected by habit. Fresh recruits going to the West Indies from

northern latitudes, almost all perish—old soldiers in the climate lose but few of their number—and natives are scarcely affected at all. The plague in a city seems to exhibit similiar variety. On its first appearance, the people are not inured to the pestilential air and they mostly perish—as the disease progresses, more patients recover; the disease continues to be milder in its attacks, till all the remaining people, who have not been affected, become less sensible to its attacks, and it gradually disappears. The small pox, a specific contagion, is very little, if at all, thus controlled by the habit of the human body.

In addition to the effect of habit in the body, there must be taken into account, a change in the temperature of the season, or state of the air. In Egypt, the plague disappears in May or June,—Alleppo, in August or September. In the northern parts of Europe, and in the United States, it increases till October, and ceases about the first of November. Heavy rains, succeeded by cool clear North West winds, arrest the progress of the disease at any time; but frost only or a degree of cold approaching the freezing point, is found effectually to destroy the activity of the poison in the air.

Why then does the plague cease in Egypt and Syria, in the midst of the hot season? This is generally attributed to the arid state of the air. It is well known that no rain falls in summer in Egypt and during the extreme heat, the air becomes so dry, that flesh will not putrefy in the air exposed to the sun. The same is true in many sandy deserts in the torrid zone.

Whether this is the true reason or not, I do not attempt to decide; but the conclusion I draw from the fact is, that the plague does not exhibit *specific* contagion; otherwise extreme heat, and cold would not check its progress. The small pox and the measles are not thus controlled by the seasons. If the weather has any effect in varying their symptoms or violence, it has no effect in lessening the certainty of the contagion: That is, persons exposed to its action, are as certain to receive infection in one season as in another. If a few solitary instances occur as exceptions, they do not in the least impeach these general principles. Their operation is as certain as the flowing of the tides or the revolutions of the day and night.

Besides the attendants on persons with the real Levant plague do not always nor generally take the disease. Russel, in his journal of that disease in Aleppo, mentions a number of instances, where the attendants and constant nurses from four to ten and fifteen in a family, almost *all* escaped infection, Does anything like this take place, among persons exposed to the small pox or measles? Certainly not.

How many, let me ask, sir, of the friends and attendants on the diseased in Philadelphia have been afterwards affected with the same distemper?

This is a question which probably cannot be answered definitely; but you will doubtless agree that many escape. I am informed that the nurses at the hospital near Philadelphia, this season, have *all* escaped infection. If this is a mistake, I wish to be corrected.

I know that in this city it was a rare thing for a nurse or attendant on the sick to be affected. This is a point on which the faculty in this city in 1795 pronounced a *unanimous* opinion. On the other hand, it was well known that a great part of the diseased had never been near a sick person. With what reason then can you decide, as you appear to in one of your letters, that the "fever is propagated by contagion and in no other way?"—Whatever may be the fact with the Philadelphia fever, I am certain from careful observation, that the most malignant forms of the yellow fever, that is, characterized with pain in the head and back, delirium, chorea, hemorrhage, a skin as yellow as saffron, and large glandular swellings, do arise in New York, without infection, and that numbers of attendants and visitants on such diseased persons, do escape without receiving the disease.

At the same time, Sir, do not believe that I am attempting to prove this disease *not infectious*. Far from it. In the sense before explained, as tending to vitiate the air, I believe the disease highly infectious. The effluvia caused by this cadaverous disease is poison to health—and when concentrated in a close room, is extremely dangerous. It is from this doctrine I propose to draw all my practical inferences. My opinion respecting the contagion of this formidable complaint is briefly this.

Specific contagion, as the small pox, acts within its sphere, independent of season or local circumstances. Cleanliness and precautions are here of no use in *preventing* the disease; tho they may mitigate its violence. The small pox will certainly infect a cleanly person in a high and airy situation, as a filthy one in a dirty house. The only way then to avoid *specific* contagion is to keep it at a distance.

Not so with the yellow fever. The power of this disease to produce itself, depends very much on circumstances over which human power has a complete control. A well person carried into a narrow street, or a room crouded with sick, or kept unwashed and unpurified and breathing the foul air for any length of time, will probably be affected with the same disease more or less. But a sick person brot into an open airy room, which is kept clean, may be visited by a hundred and a thousand persons without endangering the health of one.

If then the infection of this distressing fever, which has spread terror and death in our large towns, for some years past, depends, in any measure, on human power, and can be either wholly destroyed, or its effects mitigated, it becomes us all as good citizens to take this principle as a clue to guide us to wise and salutary measures for those purposes.

It becomes then a question for discussion how far the prevention and mitigation of this disease depend on human power. To understand this point it is necessary to examine how far the propagation of this disease, unquestionably the most dreadful that men have to encounter, is aided by *an epidemic constitution of the Atmosphere*. Without this state of the air,

favoring infection, Mr. Russel, who was a warm stickler for the specific contagion of the plague, declares the disease will not spread and become epidemic. page 307. In this opinion he is most unquestionably right; tho it totally overthrows his own doctrine of *specific* contagion; for the very definition of *specific contagion* is that it propagates a disease by contact or near approach, with certainty, and independent of auxiliary causes.

But tho not specifically contagious, the plague is infectious in all its varieties, in different degrees, according to the state of the atmosphere. This is a most important circumstance, and will be the next subject of consideration.

LETTER XIV (November 27)

It is a common observation among medical men, that a particular state of the air, denominated by some writers, an *epidemical constitution of the atmosphere*, has an influence in generating or propagating diseases. This phenomenon in the physical world, tho observed by professional men, has escaped the notice of people in general, or at least has not had its due credit among the causes of epidemic sickness. If I mistake not, Sir, this is a most influential cause of malignant complaints incident to men, and therefore I shall throw together such observations on the subject as I have made or been able to collect from others.

A constitution of air favoring the propagation of disease may be well divided into two kinds; *partial* or *local*, and *general*. The first or partial epidemic constitution of the air, I consider that which is caused by substances in particular places, which by the action of heat and water, evolve a species of air or acids, which float in the atmosphere, and render it unfit for the support of healthy life. Of this we have innumerable instances, within every man's sphere of observation or reading.

The coast of Surinam is one extensive morass, a little above the level of the ocean; the consequence is, the air is unwholesome; the Dutch ships which lay some time at that place, lose one half their crews, and for this reason, always go out double-manned.

Alexandretta, or Scanderoon, a port on the Syrian coast, is situated among marshes, in a great plain.—The consequence is the same as at Surinam. Malignant fevers always prevail there, and vessels lose one third or one half of their hands; and sometimes whole crews fall victims.

The marshes about some of the lakes in the northwestern parts of this state; the north and south ponds in Sheffield, in Massachusetts; and many other places in the northern states, together with a great portion of the flat lands, on the eastern shore of the Chesapeake, and to the southward, are examples of the same kind.

Some of these are more, and some less unfavorable to health; and the insalubrity of the air is also very much modified by the seasons, in different

years; but they all furnish examples of a *local atmosphere* highly vitiated at times, and producing epidemic fever. From the account of Dr. Buel, a judicious and observing physician in Sheffield, it appears that this noxious atmosphere in the years 1794 and 1795, extended about one mile and a half from the ponds. Its effect was very sensible and obvious; for in 1794, of 150 persons residing within the noxious atmosphere of the south pond, 80 were affected with the bilious remitting fever; and in 1795, of 200 living within the atmosphere of the north pond, 150 were affected—a prodigious proportion of persons to be ill in one summer, and far exceeding the proportion commonly affected by the yellow fever or plague in large towns. Russel observes that it is a large proportion when *one third* of the inhabitants in the Levant cities are affected with the plague in one season.

If such is the effect of the effluvia of vegetables only, in a state of decomposition, what must be the effect of human effluvia, which is still more poisonous, when generated in great abundance and united with vegetable acids in all our crouded cities. Surely, if vegetables alone can vitiate the air, so as to produce a bilious remittent in *three fourths* of the people who breathe it, in the open country, where the course of the winds is free, to dissipate the deliterious particles, is it not rational and philosophical to suppose that vegetable and animal substances united, in the crouded streets and houses of our populous cities, where continued heat for months acts freely in extricating the morbid effluvia, and where the current of winds is broken and obstructed, should produce still more fatal diseases? Must not the air of a city become necessarily vitiated in a high degree, every summer? To what other cause, Sir, will you ascribe the regular return of the cholera infantum in every large town, soon after the commencement of hot weather? This disease may vary in its violence in different years; but does it not appear, in some form or other, every year, and prove fatal to the health, and even to the lives of children, in every city? Surely, Sir, there must be a cause for this regular phenomenon; and that this cause is in the air, there can be no shadow of doubt.

That this is the true mode of accounting for the disease is even demonstrated, by the speedy recovery of children, affected with this disease, (unless in its last stages) when removed from a town into the fresh air of the country. So sudden is the salutary effect of fresh wholesome air, that a disease which baffles medical aid in town, yields in a day or two to respiration in an atmosphere not vitiated by local and artificial causes.

The conclusions from such facts are of infinite magnitude, in deciding the question between us. The chain of reasoning is this. If in ordinary seasons, the artifical sources of contagious air do generally vitiate it, in a particular place, so as to produce disease in the tender bodies of infants, and small children; the same causes, augmented to a certain degree, will so vitiate the air as to occasion disease in the firmer bodies of adults. This reasoning is to my mind, satisfactory.

Let us then attend to another step in the process. We observe children are taken ill very early in summer; usually the cholera infantum in New-York appears early in June. A short period of sultry weather appears sufficient to produce it.

But the bodies of adults do not yield so early to the impressions of heat, and a noxious air; of course bilious fevers do not appear in adults till July; and even late in that month. Exceptions may be found; but I speak of facts in general. From these facts I draw the following conclusion; that the causes of bilious complaints gradually augment during the heat of summer, of course that those causes, existing in all large towns admit of *all degrees of violence*, from the slight vitiation of air or mild action of heat, which shows its effects in the tender bodies of children, to the augmented corruption of the atmosphere which produces the bilious remittent in the soundest constitutions.

Such is the ordinary course of things in common seasons. The influence of heat acting on certain substances and bodies, in large cities, produces annually this vitiation of the air; and this is what I shall call a *partial* or *local* constitution of the air, favoring particular diseases, and rendering them epidemic. This constitution of the air is also subject to great varieties in degree, which varieties depend ordinarily on different degrees of heat, different quantities of rain and the prevalence of particular winds. According to these causes, and their different forces, the diseases of summer and autumn exhibit various degrees.

A *general* constitution of the atmosphere favoring the prevalence of particular diseases, at certain times, appears to me to be among the most influential causes of epidemics. This state of the air appears to arise from causes unknown; and its existence is observed only in its effects. That such a state of the atmosphere, extending over whole countries and a large portion of the globe, does at times exist, I can no more doubt, than I can question the revolutions of the seasons—and it is obvious that its approach is gradual, if not its decline; and its course is often progressive from place to place.

In proof of this doctrine, an immense number of facts may be collected; but I shall enumerate a few only, from authentic history and observation.

LETTER XV (November 29)

If we look into history, we observe at certain periods, without regular intervals, diseases which are usually confined to particular places, spread. without any known or apparent cause, over whole kingdoms and states, and sometimes almost over the whole earth.—We read of the plague in Egypt in the time of Moses; but we hear of no extension of that disease over Greece till the time of Thucydides, when the disease spread over Egypt, Lybia, Greece, Macedonia and Persia. No rational cause can be assigned for the extension

of that distemper, at that time, rather than at any other, except a particular state of the atmosphere.

In the reign of the Emperor Justinian, A. D. 542, the plague broke out in Egypt, near Pelusium, between the Serbonian Bog and the eastern branch of the Nile, about the lat. 31°. This was probably the most general and destructive pestilence that ever ravaged the world. It spread eastward over Syria and Persia; westward along the coast of Africa, and all over Europe; and continued to rage with more or less violence for fifty-two years. The authors who relate the history of it speak of the *myriads* of the human race that fell victims to it. This plague, like that of 1760 in Syria, and like the same disease in America, was most fatal to young and sound constitutions. The symptoms exactly the same as we observe in the violent cases of yellow fever. Writers alledge that it was propagated by the intercourse of armies and of trade—Procopius, however, asserts it was not contagious. Other contemporary writers contend for its contagion. But the historian from whom I take this account declares, that " unless the atmosphere is previously disposed to receive the disease, it would soon expire in the cold or temperate climate." This is the only possible way to account for the calamitous effects of the disease at one time, when at other times, its power of action is limited to the unwholesome spot where it originates.—See Gibbons hist. vol. 4, ch. 43. This author ascribes the origin of the plague to putrifying animal substances, in damp, hot, stagnant air.

In the reign of Marcus Aurelius, a plague raged in all the provinces of the Roman Empire. In the year 430, it almost depopulated Britain. But the plague which raged in Europe in 1348, was one of the most general and fatal, that ever was known. This was said to have originated in China in 1346, and to have travelled westward thro Persia, Turkey, Greece, Egypt, and Europe in general. It appeared in England in 1348, raging with terrible mortality, and spread into Scotland, Wales and Ireland. The harvest was lost for want of hands to gather it. It proved most mortal to the poor and to old men, women and children. Few, says Smollet, of the better sort died. After it subsided among men, it carried off vast numbers of sheep and cattle, and no birds of prey would touch their carcases.*

What should occasion this malady to spread over the habitable world at one time and not at another? It is often prevalent in Egypt—and before the great fire in London in 1665, that city was infested nearly one half the preceding hundred years. In Egypt and in London it doubtless arose from a local predisposing influence of the air; or, if it was imported into London, this constitution of the air of that city, was the means of rendering it epidemic. But how comes it that the plague, in ordinary years, and for twelve years together, should be confined to London, and never spread out of the city; yet another time, and without any apparent cause, should spread over the

* A similar fact is related by Thucydides of the birds, in the plague in Athens.

whole country and almost lay it desolate? There must be a cause adequate to this extensive effect—and we know of none but the constitution of the atmosphere, because there is no common medium of communication, except the air.

That the cause of this effect is in the air, is rendered probable by another consideration, that the more extensive is a malignant disease, the more violent are its symptoms, and the more fatal. This indicates a state of air, predisposing the human body to the action of the fever, or rather unfitting it to resist its effects.

The large cities in the Levant are almost every year troubled with the plague. Constantinople is said rarely to be free from it—Smyrna is very subject to it—The islands in the Archipelago and the Syrian coast, are not often infested. But in the years 1759 and '60 the plague broke out in all quarters, and spread desolation over Cyprus, Aleppo and all the coast of Syria. What visible cause can be assigned for this extension of the malady in those particular years, in a country that had been free from it, as the Syrian coast had, for 30 years preceding? If the disease is propagated by infection only, why should it never appear in scattering towns and cities, in other years; especially when it is known to exist almost constantly in Constantinople and Egypt, and an uninterrupted intercourse of trade is maintained between infected and uninfected places? Can it be rationally ascribed to accident or to the laws of quarantine, that a whole country should escape for 30 years together, and all at once be assailed with the disease, without any visible cause?

I frankly own, Sir, that I can resolve the effect into no other cause than the general state of the atmosphere.

It has been frequently asked, if the malignant disease which has prevailed in the United States, for some years, is of domestic origin, why it has not appeared before 1793. On my principles, I should answer—It *has* appeared before, in particular parts of America where local causes might have generated an atmosphere to favor its generation or propagation; if imported. But until the year 1793 no general Epidemic state of the air in America has existed thus to originate or propagate the disease. And this answer, Sir, would, in my mind, satisfy the inquiry. I judge, in this disease, from its analogy to a great number of others, which are recorded in history. Often, often has it happened in other countries, equally favored with a clear sky and a pure air, that for a long series of years, they have been exempted from pestilence, and all of a sudden they have like the United States, been attacked with wide spreading mortal diseases, without any visible adequate cause.

This argument, Sir, not only receives strength, but demonstration, from the prevalence of pestilential diseases among the natives of this country, before the arrival of the English settlers; and even among the brutes.

In the year 1617, three years before the first English settlers landed at Plymouth, in Massachusetts, a pestilence raged among the Indians, from

the Narragansetts to the Penobscotts, exclusive of both those tribes, and carried them off in immense numbers. I have not before me the most particular accounts of the plague; but Prince in his chronology of New England mentions it as follows. "This winter and spring ensuing a great plague befalls the natives in New England, which wasteth them exceedingly, and so many thousands of them die, that the living are not able to bury them, and their skulls and bones remain above ground at the places of their habitations for several years after." He quotes Capt. Dormer's letter in Purchas and Governor Bradford's account. No fact is better attested; Capt. Dormer himself was in the country at the time, and our ancestors found the bones of the deceased, and all the appearances of a great desolation. In addition, the natives who survived gave them an account of the frightful distemper. Was this pestilence imported?

Not many years ago, an epidemic mortal disease broke out among the natives on the island of Nantucket, which was ascribed to the badness of their corn. It raged only among the Indians, to whom it was very fatal, while the other inhabitants escaped.

Sometimes this constitution of the air exhibits most singular effects, by predisposing particular descriptions of persons only to a malignant disease. In the year of Rome 477, a plague appeared which affected none but women with child and breeding cattle. Hook, vol. 1 595. In the time of Gentiles, the plague affected very few women and scarcely any but lusty men. Many similar facts are related by undoubted authority. The fever at Sierra Leone in 1793 was most fatal to women—almost every one perishing. The yellow fever in our country has usually been most fatal to young, sound constitutions.

LETTER XVI (December 1)

The sudden extension and unusual violence of diseases are however, not the only arguments to prove an epidemic constitution of the atmosphere, favoring those particular diseases. A most decisive proof in favor of the doctrine, is, the *progressive alteration* of the air, which is visible also in its effects; I mean that a gradual change of air from better to worse, is observable in producing epidemic diseases, at first less malignant than those which are to follow, and ending with the most formidable of all distempers, the plague.

Russel, in his journal, page 9, relates that the great plague of 1759 and 1760, in Syria, was preceded by a malignant petechial fever in 1758, which became epidemic, and raged with great violence at Aleppo, carrying off great numbers of people. Indeed the fever and the famine, which prevailed at that time, are represented to have been nearly as destructive as the plague which succeeded. In 1759 the plague began to appear in Cyprus, in

Latakia, Acre, and various other parts of the Syrian coast. In 1760, the disease broke out in Jerusalem and Damascus, and at Aleppo it carried off about 500 persons. In 1761, it became epidemic, and proved fatal to upwards of seven thousand of the inhabitants of that city. From August to the beginning of the next year, its rage was suspended; but in spring it broke out with fresh violence, and in the summer of 1762 carried off eleven thousand. At the close of that year, it ceased entirely, without any apparent cause. It, however, continued in some towns to the northward, till the year 1765.

In the history of this disease, which we may believe to be accurate, we observe a regular progress of an epidemic constitution of the air over a great extent of country, producing first an epidemic petechial fever, followed by the plague, and gradually increasing in extent and violence, till suddenly it disappeared.

I know not how generally such epidemics have observed a similar progress; but the phenomena preceding the yellow fever in this country bear a striking resemblance to those of Aleppo, as described by Russel. They deserve particular notice.

In the autumn of 1789, appeared a new epidemic in this country, a severe catarrhal affection called the influenza. It had spread over the north of Europe in 1781 or 2, beginning in Russia and traveling along the shores of the Baltic westward to Holland, Great Britain and Ireland.

I do not know the precise time of its appearance in Philadelphia—I have been told it was as early as August. But its progress eastward I observed with some attention. It appeared at Hartford, in Connecticut, where I then resided, about the middle of October, and had become a subject of remark as an extraordinary kind of cold. It did not appear in Boston till the first week in November. Its appearance was progressive, and the disease might have been infectious; but its rapidity of progress precludes the supposition that it was propagated by infection; it can be resolved only into a general contagion of the air.

The next spring it appeared in March in the interior country two hundred miles from the ocean, and gradually proceeded southward, in a direction nearly contrary to that which it observed in the preceding autumn, and arrived in the sea shore towns in May. On this subject I speak with confidence, for I was on a journey that enabled me to be an eye witness of its progress from March to May, in a large district of country.

This disease deserves notice on account of its novelty, and its being the precursor of more malignant complaints. It demonstrates two points—first, that an epidemic constitution of the atmosphere may and does at times exist, as the cause or medium of spreading epidemic diseases. Secondly, that a *new* state of the air, in a particular country, may take place, and give rise or currency to diseases *new in that country*, without infection or specific contagion. And I conceive it as good reasoning to say, that the influenza

must have been imported, and propagated by specific contagion, because a *new disease* in the country, as to say the yellow fever, for the same reason, *must* have been imported.

But as a precursor or herald of more fatal epidemics, the existence and progress of the influenza, are well deserving the notice of medical men and philosophers. The state of the atmosphere, which generated that epidemic evidently appears to have been the *beginning* of a new and uncommon change in the air of our country, in the progress of which, more pestilential effects were to follow. The next grade of the disease which spread over our country was the Scarlatina anginosa, scarlet fever or putrid sore throat.

The exact time of its appearance as an epidemic in the middle states, I do not know; but in the winter of 1792, it was prevalent and fatal in New York. It moved slowly to the eastward—in 1793 it appeared in various parts of Connecticut—in 1794 and 1795, it reached Boston and New Hampshire—attended with great mortality, especially among children. But the disease exhibited most decided proofs of what I have before advanced, that the change of atmosphere which favors malignant epidemic is *progressive*—Dr. Monson of New Haven observes, in his account of the yellow fever, in my collection page 174, that in Sept. and October 1793, the autumn preceding the appearance of the yellow fever in that city, "many of the inhabitants were affected with a slight influenza, stinging pains in their jaws and limbs, soreness in the muscles of their neck, with a slight fever." In November and December, the ulcerous sore throat appeared among children, but in every instance it terminated favorably. In January 1794, it assumed a more malignant appearance; in the five succeeding months, it proved fatal. In June the same year, the yellow fever appeared—originating, as was supposed, from imported infection.

Here is displayed a *regular progression* in the virulence of the disease, which was the precurser of the most mortal of all distempers. And tho it is made probable that the first persons attacked with the plague in New Haven were tainted by the effluvia of cloths from the West Indies, yet nothing is clearer, than that the same infection, without a predisposing pestilential state of air, would have been entirely harmless. Savary remarks, from observation in Egypt, that the plague imported into that country, never communicates, unless favored by the season.

I am able to verify this account of the progressive state of the ulcerous sore throat, from observation and experience in my own family.

While that disease was raging in New York, and carrying off numbers of children, in the spring of 1793, it appeared at Hartford in a *mild form*, but as an epidemic. My own children being affected, I attended night and day to its symptoms, which were, catarrhal affection of the throat, attended with a slight external discoloration about the neck and breast, and a fever whose exacerbations were violent, but the remissions regular, attended with pro-

fuse perspiration. I saw no cases except in my own children, but the attending physicians informed me that the disease was common in the town, tho in no instances, mortal.—This was in April and May 1793. The next winter and spring, the disease assumed its more malignant symptoms, and numbers fell victims to it—The progression of that disease in malignity was extremely visible, within the sphere of my observation. Whether it was noted in other parts of the country, I am not informed.

To the scarlet fever, as an epidemic, succeeded the plague, in our cities. In some it appears the flame has been enkindled by infection from abroad. In others, the origin is questionable, or clearly domestic; but facts every where concur to prove, that in addition to local causes of diseases, there has been a predisposing influence of the air of our country, to aid their propagation.

In confirmation of this opinion, we have observed, that the common autumnal complaints of the interior country have, for some years past, either appeared in places where the people have been usually exempt from them, or have assumed unusual violence. The bilious fever about the ponds in Sheffield, says Dr. Buel, had not appeared for many years before, when in 1794 and 1795, it broke out in great extent, and unusual malignancy. The dysentery has been, in many places, much more fatal than usual, and attended with a mortality equal to that of the plague.

Facts of this kind offer irresistible evidence of the doctrine of an epidemic constitution of the atmosphere.

Postscript to Letter XVI

Since finishing the foregoing letter, I have for the first time opened the works of Dr. Mead. That celebrated author, a zealous advocate of contagion in the plague, admits the doctrine above discussed, of a state of air corrupt or pestilential, and numbers it among the causes of epidemics. But what I notice in particular, is his remarks in page 255—that just before the plague broke out in Marseilles, there might be a "fever of uncommon malignity"; and "an instance or two of fevers attended with eruptions, bearing some resemblance to those of the plague." He then adds, "for such I myself have sometimes seen in London."

I have never read more than three or four medical books, and therefore am not able to say how far it has been a subject of remark, that great and extensive plagues have been preceded by other malignant epidemics. The result of my few observations in life has been, that not much stress has been laid on this fact; and yet I strongly suspect that much light might be thrown on the causes of violent epidemics, by a series of careful observations, on their *beginning* and *progress*. It is not improbable that the state of air which favors contagion, will be found to be so *gradually progressive* that a plague may be foretold with the utmost certainty, and mankind be

taught, in large cities, to redouble their precautionary regulations to avoid the disease.

I cannot close this subject without stating another curious fact. Dr. Mead observes, "that the plague sometimes spreads very irregularly. Thuanus speaks of a plague in Italy, which one year was at Trent and Verona; the next, appeared at Venice and Padua, leaving Vicenza, an intermediate place, *untouched*; tho the next year it appeared at Vicenza." page 260.—The conclusion he draws is that the disease must have been communicated by infection and in no other way. The fact is curious, tho I do not admit this deduction.

The yellow fever has exhibited a similar irregularity in this country. In 1791 or 1792, it made its appearance in New York—it excited alarm—destroyed about 200 lives—but did not receive the appellation of plague or yellow fever. It disappeared in autumn.

In 1793 it appeared in all its horrors in Philadelphia, and no where else. New York was not only exempt, but the disease brought from Philadelphia to this city shewed no disposition to spread. In 1794, the disease passing by New York appeared at New Haven. In 1795, it became epidemic in New York and Norfolk, (I think in Baltimore also, but am not certain whether the epidemic in Baltimore was in 1794 or '95) but Philadelphia escaped. Charleston, which had escaped without any unusual sickness, during the rage of fever in Philadelphia, was severely afflicted in 1796. In 1796 also, the fever appeared in Boston and Newburyport. In the summer past, the fever has appeared at Providence and Philadelphia, and the intermediate places have escaped.

But this is not all. Dr. Buel of Sheffield, relates, that about two ponds in that town, the epidemic bilious remittent showed itself in different years; in one autumn, the fever invaded the inhabitants living near the south pond, and those about the north pond escaped. The next year, the case was reversed; the people about the north pond were assailed, and those about the other escaped. This happened, says the Doctor, without any apparent cause that he could discover. See his account of the fever in my collection.

There is something in these phenomena that is not yet explained—Dr. Mead's idea of infection as the sole cause is utterly inadmissible and the facts related by Dr. Buel of a bilious fever not infectious, which *raged by districts*, in different years, subverts his theory.

But this singular circumstance is not peculiar to the plague, or to the yellow fever. The dysentery shows itself in the same manner *locally*. In 1794, when the yellow fever raged in New Haven, the flux raged at Derby, ten miles distant, with a mortality equal nearly to the plague. In 1795, the dysentery appeared, with similar mortality, at New Haven, while other contiguous towns escaped. In 1796, the same disease was very fatal and epidemic in Worcester, Massachusetts; and the last summer, it appeared for the *first* time in Hanover, Dartmouth College, (a town that has been peopled 30

years) with uncontrollable malignity. Let any man account for the irregular appearances of this disease, in this *insulated form.* Now this deadly flux or dysentery is acknowledged by Lind and others, to be the same disease as the yellow fever, *falling on the intestines.*

If I do not misremember, the putrid sore throat a few years ago, appeared in the same manner—often, in its progress, passing by particular towns, and breaking out 20, 30, or 40 miles in advance, and some months afterwards appearing in the intermediate places. That disease is very contagious, yet nothing can be more certain than that it was not propagated generally by infection, but by an epidemic state of the air. It seized children mostly, who had no intercourse with each other, nor with infected places—I can assert this from personal observation in many instances.

Equally certain is it that the dysentery could not be carried into the interior part of New Hampshire, and propagated by infection so as to affect a whole town, without touching twenty other intermediate towns. The idea is wholly inadmissible.

Two ideas suggest themselves as a solution of this phenomenon. First, a *general state* of air, favoring these diseases, uniting with *local causes,* may well enough account for the irregularity in the progress of epidemics. Supposing the general constitution of the air of a country to be every where the same, equally pre-disposing the body to disease, then local causes may be very unequal, and give rise to a disease at one place this year and at another the next. These local causes may be marshes, great rains, drouth, scarcity of ordinary food, bad bread from unfavorable seasons, and many other accidental or occasional circumstances.

Another idea is novel, and may be whimsical. If we suppose, by a figure, the atmosphere in its ordinary state to be in health and that a pestilential state of it is a disease, why may we not suppose the pestilential fluids to be impelled together by attractive powers, like those which unite other bodies—and that of course in particular parts of the atmosphere, this cohesion may render the air vastly more noxious in some places than in others—and further that this body of pestilence thus accumulated, may be driven by storms or changes in the seasons, from place to place: an analogy like this we have in the human body, when affected with certain complaints. The disease often shifts its ground, appearing in one place with pain, and then in another, till it fixes upon some part with violence.

This last idea may be chimerical, but the first appears to be rational.

I will close this lengthy postscript with one remark. If practitioners in physic could spare more time to note facts, and make philosophical remarks, a comparison of their observations might in time lead to the solution of many of the phenomena of epidemics, which are not yet understood.

Note—: I ought to have introduced into the body of the preceding letter, an extract from Dr. Baily's account of the fever in New York in 1795. see

page 55 and onwards.—The facts there stated are in substance as follow:—That at a meeting of the Medical Society of N. York, July 2, that year, it was remarked, that several cases of angina trachealis, attended with *anomalous symptoms*, had occurred and a few cases of dysentery which proved obstinate; that these diseases were not usual at that season—and it was *predicted* that these diseases would *become epidemic, or were the forerunners of one.*

In consequence of these remarks, it was resolved by the members that they would, during that month, make particular observations for the information of the society at their next meeting. But the yellow fever pressed on the heels of its precursors too closely; for it appeared the last of that month, and the occupation of the physicians prevented the results expected from the resolution.

I submit to the faculty whether this angina trachealis in New York, bore any resemblance to the harbingers of the yellow fever in New Haven, "the stinging pains in the jaws and limbs, the soreness in the muscles of the neck, with a light fever," and the mild form of the "ulcerous sore throat," described by Dr. Monson in my collection—page 173-4. But I cannot omit to remark, that the *signs* of an *uncommon epidemic* were so visible in New York, early in the summer of 1795, as to produce a *prediction* of the event; and yet we have men who are hunting among the vessels in our harbor for some *imported infection.*

LETTER XVII (December 5)

I have a number of other facts to offer in confirmation of the opinion that there is, at certain periods, a general state of air, which favors the generation and propagation of malignant complaints. These facts and many already mentioned are not new to the professors of medicine, nor do I offer them for *your* information; but for the information of my fellow citizens at large, who have not attended to the subject, and who are all interested deeply in knowing the truth.

One of the most singular complaints that ever became epidemic among the human race, was the plague in England, called the *sweating sickness*, described, from John Kaye, or Caius, a cotemporary practitioner, in Aikin's Biographical Memoirs of Medicine, page 120.

This disease first appeared in England in 1485; afterwards in 1506. 1517, 1528 and 1551. It broke out originally in the Earl of Richmond's army, on his landing at Milford Haven. Its attack was sudden, beginning with an affection of some part of the body, like a sense of hot vapor running thro the limb. To this succeeded internal heat, unquenchable thirst, and profuse sweating, which often carried off the patient in two or three hours. The violence of the attack was over in 15 hours, and the patient reckoned out of danger in 24 hours. It carried off incredible numbers of people, where it

raged. It was most fatal to persons in high health, and better condition of life.

Notwithstanding the unusual appearance and effect of the profuse sweating, it is obvious that the disease was the real plague, as it was attended with many of its most characteristic symptoms—anxiety, restlessness, violent pain of the head, delirium and excessive drowsiness.

The writer ascribes the disease to the thick, unwholesome fogs or effluvia which arose from the low grounds in the neighborhood, aided by the foul air of close narrow streets, uncleansed drains, &c.

It has been asserted that this singular disease attacked none but the English. But this is a mistake, it raged with violence in Germany and the Low Countries in 1529 and 1530.

The use I make of this fact, is, that it is no objection to the domestic origin of a complaint, that it is *new* or has never before been generated in a particular country. The peculiar symptoms of the sweating plague first appeared in England in 1485, and disappeared after the year 1551, a period of less than 70 years. Those symptoms of the disease originated in that country, and never extended beyond that and the neighboring countries nearly in the same latitude. Facts of this kind are not to be reasoned away; and they prove decisively the possibility of great changes in the constitution of the air, or in the bodies of men. If the changes are in the human body, they must be wrought by the access of *causes*; and what can be the causes, but the diet or alterations in the air. If we attribute the change to an alteration in the essential properties of the diet this alteration can only be resolved into a change of seasons, affecting the air and water, the food of plants. So that we are led, by this process of reasoning, to the same result.

What are the causes or what the essence of the changes in the atmosphere, we know not. It is well known that the air is a compound fluid; that some of its elementary parts are salubrious, others fatal to life. We perceive constant changes in the *weight* and *proportion of respirable air*, in that fluid,—and it is to be presumed, that the vast changes in the temperature of the air, heat, cold and moisture must occasion great and general alterations in the composition of the elementary parts. Perhaps too the electrical fluid, that most powerful agent in the physical world, may, in its action, produce essential changes in the air. Something of this kind appears probable from the frequency of earthquakes in countries subject to them, and of unusual tempests, which often precede, attend or follow, the plagues which become general or of great extent.

But a most substantial proof of the constitution of atmosphere for which I am contending, is that plagues, or mortal epidemics prevail at times, among the brutes. It is a confirmatory fact, that most fatal epidemics are preceded by uncommon seasons. Livy. lib 13. Ca. 13 relates that a remarkably severe winter in Rome, in which the navigation of the Tyber was obstructed by ice, and the roads rendered impassable with deep snow, was followed by an extremely hot summer, and a dreadful plague which raged equally among men and *cattle*.

The great plague in Turkey in 1751, which destroyed one third of the people in Constantinople, was foretold by the inhabitants, from the uncommon snows of the preceding winter—a prediction founded on experience. Chenier's Morocco, vol. 2. 275. Superstition has, in the Levant, connected the plague with many phenomena, and the influence of the planets. I pay no regard to popular notions any farther than a connection between cause and effect will authorize them; but I draw this conclusion from a multitude of facts, that violent and general epidemic diseases are connected, in their origin and propagation, with important visible changes of the seasons, and therefore it is fairly persumable, that important and influential alterations in the atmosphere do in fact favor the origin and prevalence of such distempers.

The great plague in 1348 was accompanied with an unusual mortality among the sheep and cattle in England. Fernelius informs that in 1514 a plague invaded Cats only. Encyclopedia *art.* plague. It is well known that the year after the plague in Philadelphia, in 1793, an epidemic disease carried off a prodigious number of horses in New Jersey.

The past summer has also given a most extraordinary proof of the doctrine here advanced. As early as April last, and perhaps earlier, a plague invaded the Cats in England, and immense numbers died. A similar mortality has prevailed among the Cats in this country. The epidemic originated to the southward of this city, and gradually advanced eastward and northward thro the United States, sweeping away those domestic animals in prodigious numbers.

It has been said and probably is a fact, that an infinite number of dead fish was seen last summer floating down James River, in Virginia, which could be accounted for on no principle, but that of a mortal epidemic which had destroyed them.—To what cause can this phenomenon be attributed, but to some unusual, deleterious quality in the elements in which they live?

It is to be wished that the beginning and the progress of Epidemics had been more carefully noted by writers. But those striking circumstances which I have enumerated are sufficient to show, that the origin and propagation of uncommon diseases are greatly influenced by a particular state of the air. It is an agreed point among several medical and philosophical authors whose works I have read that the plague even in the Levant, does not spread by infection without a state of air favorable to its propagation. Savary testifies, that infected persons had been introduced into Egypt and died with the plague, without the least appearance of infecting others. Russel, page 4, gives a remarkable instance of a similar kind. In 1759, as the great plague was beginning to make its appearance in the Levant, the disease was introduced into Limsol, a port of the south side of Cyprus, where it spread and was mortal. Larnica, a town forty miles distant, received part of the infected crew, that brot it to Limsol. A constant communication was maintained between the two cities; peasants and mule-drivers entered Larnica, with their

pestilential sores upon them, and were daily in the streets and markets. Some of these people died in the houses; and other vessels from Egypt arrived with infected crews, some of which died on landing—yet, strange as it may appear, no person in Larnica was affected nor did the disease appear there till the next January, a period of eight months. Then it broke out and made havoc among the inhabitants. The fact is curious, and can hardly be accounted for, without recurring to an epidemic constitution of the air.

The fact however is the more credible, as it corresponds with what we have all seen in this country. In 1793 and 1797 several persons from Philadelphia have arrived at New York, either ill or soon after arrival, seized with the disease prevailing in that city; and no infection appeared among the people of this city. Yet in another year, and a year too when the disease did *not* prevail at Philadelphia, the disease broke out and became epidemic, without any infection from abroad that could be traced. Is it possible, Sir, to account for these facts on the principle of specific contagion?

Since the foregoing was written, I took up, by accident Thompsons Travels thro Egypt in 1734 in which there is a very good account of that country. On the subject of the plague the author has the following observations.

"The plague is not an annual disease in Egypt, nor does it occur often, unless the infection be brot from Constantinople or Alexandria.

When it breeds in Egypt, it is usually in February, the coldest month. (but the weather in that country is never very cold.) It rages violently during the hot southerly winds, but ceases entirely with the rising of the Nile and the access of the North winds. If the infection comes from Greece, it seizes many, but its *symptoms are mild, and it seldom proves mortal.* When it is derived from Syria, it is more fatal for tho few are seized, more die than in the former case. But when the disease is brot by the Caravans from Barbary or Ethiopia, which sometimes happens, it is worse than from any other quarter, laying all Cairo waste; preying upon all degrees of people with grievous and frightful, symptoms which generally terminate in the death of the infected. It is said that six or seven thousand have died in a day; but this rarely happens, it being a large number if 70,000 die in a year.

The plague, says this author begins with chilliness and shivering, *like the access of an intermitting fever*; then comes on a nausea with vehement vomitings, an intense pain about the region of the heart; and a burning fever, which continually preys on the patient till death or the eruption of some bubo, parotis, or other tumor relieves him, by discharging the matter of the disease. Sometimes indeed, it attacks, without any fever, purple spots appearing all at once, the certain sign of present death; but this rarely happens; *except at the beginning of some terrible pestilence.* It has also been known to appear first in tumors, without any fever or violent symptoms. Heaviness, pain in the stomach, head and back, cardialgy, broken sleep,

anxiety, difficulty of breathing, alteration in the countenance, hicup, syncope, delirium, convulsive twitchings, diarrhea, eyes sunk or inflamed, tongue black and dry, vehement thirst, fetid breath, carbuncles and spots of various colors, are also symptoms commonly attending this deadly disease." Vol. 2d p. 193 duodecimo.

The conclusions from the foregoing facts are that the plague invades Egypt from all points of the compass and also originates in the country—that it exhibits various degrees of malignity and sometimes is not very mortal—that its symptoms vary in some degree, but in general correspond, exactly with those of our yellow fever—that its most violent and rapid action is seldom seen except at the beginning of some *terrible pestilence*—that it begins in February, from 8-10 weeks after the Nile has left the plains and settled into its bed—and that the fresh winds from the north, over the Mediteranean and the fresh waters of the Nile washing from the low lands, all the filth, put an end to the disease.

These can be no doubt of the truth of the facts nor of the deductions from them—Mr. Volney remarks that it is " easy to mistake malignant fevers in Egypt for the plague." The *malignant fevers* of Volney, are probably what Thompson calls the plague with *mild symptoms and not mortal.* The Egyptians suppose *this mild plague comes from Greece.* The whole truth probably is, that in *ordinary seasons*, the disease is *mild*—in less favorable seasons, it assumes more malignant symptoms; but when aided by the very powerful influence of an *epidemic state of the atmosphere*, it becomes what Thompson calls a *terrible pestilence*, and carries off thousands in a day.—I believe the same to be true of the disease in all other countries.

LETTER XVIII (December 6)

Facts and remarks supplementary to the preceding Letters.

The crews of vessels bound from northern latitudes to the southern, usually remain healthy, till the vessels make a port and anchor in some sickly place. They are then attacked with the diseases common in the climate. Lind, page 52. This is a proof that heat, even in the torrid zone, is not the sole cause of fevers. It is *not climate, but substances on land*, that cause disease; or crouded holds of ships and neglect of cleanliness.

Dr. Hunter is express to my purpose. He declares " that the production of noxious vapors depends on the concurrence of three circumstances, heat, moisture and decayed vegetable or animal matter. The heat of tropical climates will not alone produce fevers. Simple moisture also is harmless; and vegetable and animal matter do not corrupt, and putrefy without heat and moisture."

The following facts from Dr. Hunter are too much in point to be omitted,

page 16, he says, "If any doubts are entertained that the exhalations from wet and marshy grounds are the causes of the fevers in Jamaica, attention to the following facts cannot fail to remove them. Ships lying at Port Royal, with their men in perfect health, on moving higher up the harbor, either opposite to Kingston, Rock Fort or beyond them and taking their stations in any of those places, have in a few days become sickly. The men have been seized with fevers, owing to the low swampy lands along the shore and at the head of the harbor, from which last the exhalations are carried every morning towards the ships, when the regular sea breeze sets in, as sensibly perceived by the bad smell which accompanies it.

"In the year 1782, two frigates moored at the head of the harbor, to guard against an attack in that quarter, were obliged to leave their station in a fortnight on account of sickness.—The men employed in filling the water casks are taken sick, either at the time, or a few days after; and there are examples, where out of 60 or 70 men sent on that duty, not one has escaped a fever."

Dr. Hunter further observes that the fevers that prevail at that place are "principally of the intermitting and remitting kind; the last is most frequent and most fatal."

This Sir, is the true West India fever, that makes such terrible havoc among strangers. And the testimony of this experienced physician is direct, that this disease *does* proceed from marshy grounds or *vegetable effluvia.* Vessels lying in the outer road, at a distance from the marshes escape the fever—as soon as they anchor near the low grounds opposite Kingston and Rock Fort, their crews are taken sick, with the fever. Call it contagious or not, as you please—but the fact cannot be doubted—That vegetables in a state of putrefaction make crews sick and many of them die. The fact *there,* is as it appears in New York—Men who breathe the impure air of low and filthy places, get sick—people in health if they go and visit them, and breathe the same air, get sick also—but carry the sick into pure air, and they mostly recover, and people who nurse and visit them in *that pure, wholesome air,* do *not* get sick. And I will venture to assert with confidence, that as a general observation, that is the truth, and the *whole truth* as it respects the contagion of the yellow fever.

Stagnation in air and water render them unwholesome. Little do men imagine how much the purity of both depend on action. Lind remarks in a note, page 53, that in long calms, near the equator, the sea is covered with a scum and becomes offensive, so that men going on deck are frequently seized with sickness and vomiting. If agitation is so necessary to the purity of salt water, what must be the effect of stagnation in fresh water! I suspect the subterraneous water in lake plains is more affected in this way, than is commonly imagined. On the other hand, the water in a hilly country is perpetually in rapid motion, descending either on the surface of the earth, or in subterranean passages; and is thus kept perfectly wholesome.

Hungary is mostly a flat country. Its rivers often overflow large tracts of land and leave lakes or stagnant ponds of water, with extensive marshes. The consequence is Hungary is the most sickly country in Europe. The Christian armies, in the crusades, often lost half their number, only in marching thro that country. The epidemics begin to prevail every year in July and continue till September or till frost. The diseases are the *same* as those of the coast of Guinea and of the East and West Indies. Lind, page 59. With this authority before your eyes Dr. Currie, how could you venture to suggest that these malignant fevers are not generated in *our climate*, when they are constantly in Hungary, several degrees *north* of our latitude, and in an inland country, that has no port on the ocean?

In Hungary it is observed that the nearer a city is to a morass or river with foul oozy banks, the more sickly are the people.

Another fact.—In such places, in the hot seasons, the air abounds with insects and animalcules, a sure sign, says Lind, of its malignant quality.

I think proper here to remark, that in the year 1795; when the weather was excessively hot, and the air humid, about the time the yellow fever made its appearance in this city, it was observed the flies disappeared and musquetoes invaded us in numberless swarms. Flies delight in a hot dry air—musquetoes in a hot moist air, and wherever the latter abound, it may be considered as certain, that the air is not salubrious. It is confidently said that the air in the city in 1795, was so pestilential, that numbers of birds perished in their cages. I cannot attest this by my own observations, but the season was by far the most unfavorable to health, that we have experienced for years,—every part of the city was loaded with filth.

The fens in different countries of England lat. 51 to 53 are known to be very dangerous to the health of those that live near them and still more so to strangers. Lind, page 66.

The noxious vapors from marshes do not extend a great distance before they are so dissipated as to be harmless. The distance however depends on the course of the winds. The farther vessels sail up the rivers in Africa the more sickly are the crews. Those that anchor two or three leagues from land are for the most part healthy. Lind p. 67.

In 1744, the Austrian army under prince Lobocowitz, near the marshy grounds about Rome, became so sickly, that they were obliged to decamp. Ships of war that lay in the Tyber, near land, became infected but those which lay at a distance escaped. Lind 69.

Lind says, page 153. when a malignant fever was carried from England to the Hospital in Minorca (how is this Dr. Currie, a malignant fever carried from a northern to a more southern climate!) the house being found insufficient to receive so many patients, tents were erected in the fields for many of the men. It was thot at first these men in tents were badly accommodated; but it proved that most of them recovered; while of the sick in the hospital,

not one in three escaped. This is, in short, in the close rooms of the hospital, the disease was *contagious* and *mortal*; in the open air, it was neither one nor the other.

The same author, page 351, recommends that persons *very ill* with jail distemper should be separated from those who have *slight attacks* only; by which the sphere of contagion will be greatly contracted; Long observation has taught him "that patients with a slight infection, when kept perfectly clean, and in well aired apartments, *do not communicate the disease.*"

A most important inference follows, which is, that infection is capable of existing in all degrees of force from the slightest impurity of air, to a pestilential atmosphere which instantly occasions vomiting in a well person that enters it—The ships of the Brest fleet from Louisburg in 1757 afforded proofs of the latter, or most virulent infection. The result is, that human power has control over this infection of malignant fevers, and may by purifications, washings, fresh clothes and free circulation of air, reduce its force and activity and finally destroy it.

When La Mothe's fleet left Louisburg October 20, 1757, 400 men were left so very ill as to be supposed past recovery. On the other hand about a thousand convalescents were embarked on board the fleet. Note the consequence. In six days, most of the convalescents were dead; and others in the fleet taken ill—but of the 400 men left on shore, whose cases were supposed desperate, 380 recovered. What can more strikingly illustrate the truth for which I contend, that contagion of this kind is not *specific*, but depends on place and circumstances. *Fresh air destroys it*—it does not indeed speedily remove infection from clothes and wood, as Lind well remarks; but it speedily dissipates the effluvia from the sick and renders it harmless at a small distance.

This experienced author observed that clothes which had been worn by persons in malignant fevers retained the infection a long time, and that this infection was more virulent and certain in its effects, than the direct effluvia or stench of the sick—an important fact.

Lind observed a number of cases of yellow fever generated in infected ships and hospitals in England. How then could you infer, Sir, that the yellow fever is *not* generated in *our* climate, ten degrees to the southward of England; but is always brot from the West Indies? The very authority you rely on to support your opinions utterly subverts your theory.

The occurrence of the yellow fever in Boston in August 1796, without the most distant suspicion of infection from abroad, is a fact which cannot be resisted. Boston is far north of New York and Philadelphia and what is a more important circumstance, its sea breezes are from the east—(not from the south) and of course much cooler than with us.—The fever first appeared in the south east part of the town, on the border of a large flat, which is bare at low water—and in a position where a family would rarely or never be

exposed to infection from the West Indies, as few foreign vessels ever approach near enough to the place to communicate infection.

The persons first seized were two women, within 12 hours of each other—they died on the fourth day. Women who had no connection with vessels would not be likely to be the first to take infection.

The parts of the town where the disease most prevailed were on the wharves and in crouded places, exposed to the stench of filthy substances, and destitute of free air. About 30 persons died, and Dr. Warren states that the "physicians of the town were unequivocally of the opinion that the fever originated from local causes." See his letter to Secretary of the American Academy of Arts and Sciences. Medical Repository page 136.

The town of Newburyport affords another proof, if possible more decisive of the domestic origin of the yellow fever, than the town of Boston. From Nicholas Pike, Esq. a gentleman of the most unquestionable candor and veracity, a magistrate of that town, I have the following account of the origin of the fever in that town, letter dated Nov. 3, 1797.

1st. "A vessel arrived which had lost two men with the fever, and a few persons supposed it imported, but nineteen twentieths were of a different opinion, because the clothes of the deceased were thrown over board, the vessel cleansed at sea, and every part exposed was planed; and when she arrived in the river, was cleansed again. Moreover *no person who worked on board her, after her arrival, caught the distemper.*

2. The situation where the fever began, was in Water street, near the wharves, to the westward of State street. The buildings adjoining the wharves are mostly contiguous, only here and there an opening.

3. There had been, previous to the sickness, a vast quantity, many tons, of Pohaden (or Manhaden) a fish about the size of a large mackerel, caught in the river for the West India market. A gondalo might be loaded in an hour or two—Vast quantities had been opened and their entrails left on the wharf and in the dock, which had bred maggots nearly as large as a goose quill, and from which such a stench proceeded that the neighbors could not bear to have their windows opened. And a gentleman informed me, that when the wind was easterly he could not pass by one of the openings between the buildings, without holding his breath. And a small house, from which three women and a young man died, was afterwards found to be so filthy that maggots were found under the floor.

The first person infected and that died, was, I think, a truckman—then two of the women just mentioned—then people of every condition, and the infection of every person, except one or two was easily traced from that street and very near the infected spot—*Almost all the attendants on the sick in that street died.* But after some time, persons were seized in other parts of the town who had either removed from that street or had watched there, and died with the disease; but *not one of their nurses caught it or not more*

than one—which I think a demonstration that the contagion was occasioned by the filth. The man who owned the wharf where the filth lay, and who fed his swine with it, and his brother who lived with him, died. They were both abroad, tho unwell, the day before their deaths!"

I submit it, Dr. Currie, to your own judgment, whether it is possible to imagine a series of facts more directly subversive of your doctrines of the *origin* and *contagious nature* of the fever in question, than those stated in the preceding narrative. A disease as virulent and active as the Levant plague sweeping away lives in a few hours, originating in the putrid garbage of fish on a wharf—and raging along the low streets—nurses and attendants in that *low spot,* taking the disease—but attendants on infected persons, when carried upon high grounds, out of the reach of the *local atmosphere.* almost all escaping. The analogy between these facts, and what we see often in New York, is perfect, and leaves no room to cavil either about the origin of the disease or the nature of its contagion.

We have direct proof of the origin of the yellow fever, with all its malignant symptoms, in Boston and Newburyport, which are in the forty third degree of latitude; and yet we have men of respectability contending that this disease does not and cannot originate in the fortieth degree of latitude. I cannot help calling this a *prejudice,* I am persuaded that a more general view of the subject will ultimately succeed in destroying it.

LETTER XIX (December 8)

The following instances of the deleterious effects of stagnant air, generated in confined places, are too remarkable to be omitted.

A few years ago, a Spanish Register ship, making the harbor of New York in distress, was stranded on what is called the West Bank. She was addressed to Robert Lenox, Esq. of this city, who gives me the following relation.

The ship was large, between 6 and 700 tons, laden with indigo, mahogany, snuff in hides, and other articles usually brot from the coast of Brazil. In order to get off the ship, she was lightened; and at a low water, a vessel on each side made fast to her.

It must be remarked, that while she lay dead on the sand, persons had entered even her lower hold, without inconvenience. Mr. Lenox himself went into her lower hold, and walked on the wood, as it floated in the water, which was so deep that a man could only pass along the floating timber, stooping.

No sooner did the tide rise so as to render the ship partly buoyant, than she began to roll with the agitation of the water. At this moment a steam of mephitic air arose from her lower hold; a man attempting to descend, no

sooner reached the steam, than he fell instantly dead—another moved by compassion, went to his relief, and as suddenly shared his fate; others followed, in their eagerness and haste, to relieve their fellow laborers, and *five* lay dead, before Mr. Lenox could restrain their ardor. Seven or eight others suffered much, but were recovered. So instantaneous and decisive was the effect of the deadly poison on the men, who first adventured down, that they fell, as suddenly as if struck dead by lightning and showed no symptoms of animation.

This melancholly event put a stop to the attempts to get off the ship at that time, and she was suffered to relapse into her old situation. After lying at *rest* a day or two, and without any measures to ventilate her, men entered her hold in safety. Hence it is evident that the corrupt air is heavier than common air, and subsides near the earth or water.

This important fact shows how extremely vitious stagnant air will become, when confined for a long time, especially when in contact with water and vegetable substances, and acted by heat. Heat indeed need not be very great, for the same state of air is often found at or near the bottom of wells, where the temperature is 47° to 52° by Farenheit. This fact indeed goes to prove, that stagnation in air alone, or at least that stagnant air, in contact with water only, will generate the same deadly poison. An instance happened in Pine street, in a new well at the sugar house, in 1795; two men were killed in the well, where there was no animal or vegetable substances. The mephitic air lay near the bottom of the well, or rather near the water.

The fact is of infinite importance. If such a poison is extracted from air and water alone in a state of stagnation, what important lessons are furnished for practical use! How nearly do all close unventilated cellars, and even backyards and narrow streets, and especially underground rooms, resemble the well and hold of a ship! Surely, Sir, we are not authorized to say that the air of our climate is incapable of corruption, or that the same physical laws do not operate in the United States, which produce their natural effect in all other parts of the world.

Another fact I have to relate which is direct to my purpose.

In the month of July, or beginning of August, 1795, Mr. Daniel Phenix, Treasurer of this city, in company with his two sons, opened some large bundles of the small tickets or city notes which circulate in New York as small change, in lieu of cents. These notes after being used till they are torn or defaced, are returned to the treasury of the city, and exchanged for new ones.

At the time mentioned, Mr. Phenix, informs me, he had a large quantity, he believes thirty pounds, which had been a long time wrapped close in papers, inconsiderable bundles. These packages, were opened and the bills counted by the Treasurer and his two sons, who all perceived at the time an offensive smell from the bills. It will be readily imagined what a quantity

of effluvia must be contracted by small bills on a thick paper, passing continually thro dirty hands, among hucksters and butchers.

The effect of this poisonous effluvia was the same as of all other foul air.—Within four days after they had opened the bills, Mr. Phenix and his sons were all seized with a bilious fever, which was severe, tho not fatal. It is the opinion of this gentleman, whose character for integrity and candor places him beyond the suspicion of any improper biass, that the effluvia from the bills was the direct cause of his illness and that of his sons.

But what confirms him in this opinion and indeed seems to put the fact beyond question, is that several weeks after his recovery, in September following, he entered upon the same task of counting the bills in another bundle, which had also been tied up for some time. The same offensive smell was perceived, and after some time Mr. Phenix perceiving a disagreeable sensation in his stomach, left the business unfinished; and drank brandy and water.—the nausea however did not leave him, till he had taken a dose of castor oil.

What will you reply to this fact, Dr. Currie? Deny it you cannot; and how will you evade the conclusions that necessarily result from it? You admit that animal and vegetable effluvia produce disease, in the holds of vessels, in the West Indies; but you seem to deny that those causes act with force enough in *our climate* to produce the same effect; and even use this strange language, that if vegetable effluvia will cause mortal and contagious diseases in this country, it is time to abandon it.

Yet here is an instance in New York, where a bundle of penny bills of the size of a half bushel, produced a bilious remittent in three of our worthy citizens, which came near to cost them their lives. This effect of foul air, Sir, is experienced in *our climate*; Yet I cannot agree with you, that on this account, it is time to abandon the city of New York—I draw a very different inference from the fact; that knowing and believing the fact, we ought to destroy the sources of disease, when we can; and when we cannot, as in case of marshes, which cannot be drained, we ought to remove beyond the region of their pestiferous atmosphere.

Another fact equally pertinent. In Dr. Bailey's account of the fever in this city, you will find a statement of the origin of bilious fever in a family in Hartford from a quantity of putrid cabbage in a cellar, which had been overlooked and neglected. If such an effect could proceed from so small a cause, why is it irrational to suppose epidemic fevers to originate in populous towns, where every cause of disease, heat, filth, animals and vegetable effluvia, stagnant air and water, concur to produce the effect?—Philosophy and reason, in this cause oppose your inferences, and repeated melancholy experience demonstrates their falacy.

No, Sir, it does not appear that *our climate*, or *our country* can lay claim to any exemption from the calamities incident to other climates, and other

regions of the earth. The same causes of disease exist in the United States, which exist in Italy, Turkey, Syria and Egypt, varied perhaps in force, but not in essence. Heat and rain will call the seeds of disease into life; and the bodies of men, in our temperate climate, exposed annually to all the vicissitudes of season, are infinitely more irritable and susceptible of the action of the deleterious miasmata, than the bodies of natives in the torrid zone. We are then in the same situation as to exposure to malignant epidemics, as the ancient Greeks, and Romans, and the modern Turks. Shall we imitate their stupidity? The ancients believed those epidemics to be sent by the Gods—They acted accordingly—They consulted the oracles—imported Esculapius in the form of a serpent from Epidaurus to Rome—buried a vestal virgin alive, and created a dictator to drive a nail into the wall of Jupiter's temple—and *all to no purpose!*

The Turks are predestinarians—they believe it is God's will to send the plague—there ends their enquiry—and there, the means of prevention.

Shall we, Sir, forever be the dupes of ignorance and superstition? What is it but superstition, that is tracing an epidemic of our cities to some *distant region*? And in what, Sir, does the difference consist, between the Greeks and Romans, who charged the plague to the God—the Mahometans, who ascribe it to fate, and the Americans who charge it to the West Indies?

Nay, is it not most ludicrous to see the English physicians unanimously fathering all plagues upon Egypt—the French philosophers, acquitting Egypt, and charging the whole source of this calamity, upon Constantinople or Smyrna—and even the West Indians deriving their pestilential fevers from the Coast of Africa? To me, this appears a ludicrous farce.

Did you never, Sir, know a parent, when some mischief has been done in the family, call round him his children and the domestics, to inquire for the author of it? Have you not heard John deny that *he* did it—Tom deny that *he* did it—Ann deny that *she* did it? Moll at last steps up, with confidence and says, Sir, we did not any of us do the mischief, but it was Dick, that lives over the way. *He* did it. *He is always doing such roguish tricks.*

Yes, Sir, it appears to me that philosophers and physicians and most other men have treated malignant epidemics, just as children conduct respecting the mischief in the family—They are forever casting off the censure from themselves upon their neighbors.

Surely, sir, the Americans ought to be the wiser; and instead of trying to shift off the origin of the plague upon the West Indies as the ancients did upon their gods, and the Turks do upon destiny, we should examine into the causes, with the eye of reason and philosophy, and if we find the causes of malignant disease in our country, why should we not admit, that those causes, may be so modified in their action in different seasons, as to produce various degrees, and the *highest degree*, of malignity? If we admit the malignant sore throat and the dysentery, two infectious fevers of nearly the same

malignancy as the yellow fever, to be diseases of our climate; how can we refuse to admit, that some accidental aggravation of the causes, may generate the next grade of disease, the yellow fever?

—To me, sir, these deductions, would, independent of fact, appear not only rational, but extremely natural. I should conclude on abstract principles, that a climate capable of generating the *second* grade of malignant disease, would also be capable of producing the *first*. But the evidence of my senses is paramount to reasoning, and leaves not one particle of doubt, of the domestic origin of the yellow fever, on the mind of

Sir, your obedient servant,
N. Webster, jun.

LETTER XX (December 9)

I have in the preceding letter, sir, stated such facts and observations, respecting the origin and progress of malignant complaints, as have occurred to me, without the labor of a regular investigation of the subject. From those facts and observations I draw the following deductions.

1. That the effluvia from vegetable and animal substances, in a state of fermentation or putrefaction, are unfriendly to health and, if sufficiently concentrated, will produce disease in *all climates.*

2. That altho animal effluvia may be most poisonous, yet that vegetable effluvia may also be so concentrated as to produce malignant epidemics. Witness the Lake Fever.

3. That from the best evidence on record, it appears the great plagues of antiquity originated in cities and camps from *vitiated air*. Witness Athens, Rome and Syracuse, London is a still more remarkable instance.

4. That the contagion of the yellow fever or plague is not *specific*; but dependent on the time, place and circumstances of the disease; or rather on the quantity of deleterious effluvia generated and stagnating about the diseased —therefore its contagious quality is very much within the control of human power. Witness all the facts relative to the fever in New York city, and in the islands before, and in the Lazaretto—also in the Philadelphia Hospital.

5. That Dr. Chisholm's system of the fever from Bulama's being a new disease in the West Indies, is wholly groundless—as the Grenada fever was not probably derived from Africa—and if it was a similar fever has prevailed in the islands before, and in many parts of America. Witness the authorities you mention, Leamy, Lind, Redman; with Doctors Taylor and Hansford.

6. That malignant diseases, even when contagious and infectious, do not spread and become epidemic, without a state of air favorable to their propagation. Witness the facts stated, and medical writers of the first eminence in all ages.

7. That a partial or local constitution of air favorable to disease may arise from substances on the earth, and that therefore this cause of malignant diseases, is very much within the command of human power.

8. But, that a general state of the atmosphere favoring diseases, proceeds from some changes of seasons or the operation of other general laws of nature, which are beyond human control and lastly.

9. That the appearance of a new disease in a particular country, is no well founded objection to its origin in that country; as the fact may well be accounted for on the general principle of alterations in the state of air, and many instances of such new diseases in particular countries are on the records of history. Witness the first plague in Greece and Rome, and the sweating sickness in England; as also the influenza in this country, and the black vomit in Carthagena.

It may be added that the destructive plague among the natives in New England, before the settlement of that country by the English is a decisive proof that plagues *may originate from local causes in our country.*

I know not, sir, whether the facts stated and the deductions from them will be admitted on your part nor whether they will make any impression on the minds of my fellow citizens. For my own part, I am firmly convinced that the formidable disease which has disturbed the tranquility and suspended the business of our commercial towns, for a number of years past, *may be* and in some instances, *has been*, generated in this country. I firmly believe that causes exist in the U. States, adequate to such an effect.

At the same time I perfectly agree with you, that the terrible disease is often generated in the foul air, on board of ships, in hot weather; and that in such cases, danger is to be apprehended from a free admission of vessels from the torrid zone, and even from other climates, after long passages without performing quarantine—I conceive that the air of all our cities is, during the summer months, extremely vitiated,—its effects first on children in producing the cholera infantum, and lastly on adults, in producing remitting fevers, demonstrate to my mind, that, in most favorable seasons, the vitiation of the air is nearly sufficient to produce more alarming maladies.

It is therefore the duty of the magistrate to enforce all the regulations subjecting vessels to quarantine or to effectual cleansing; that no deadly poison from abroad may be introduced into our cities, to augment the local causes of disease.

The application of the foregoing doctrine to practical purposes, I shall at present postpone, and close this letter with the following argument.

We observe in extensive plains, tho dry, more autumnal complaints than on hills. This may well be attributed to stagnation both of the air and water. Circulation in both is essential to their purity.—A dry extensive plain I consider as the first or slightest cause of autumnal epidemics.

The next grade in the scale, I suppose to be small marshes or stagnant water pools. Here we see, almost every year, slight intermittents.

Causes of the third grade producing epidemics, are extensive marshes, where stagnant water is filled with vegetable substances. Near these we see, in ordinary seasons, intermittents, and in unfavorable seasons, remittents.

The next place in the scale may be occupied by our largest cities in healthy seites which, in ordinary seasons produce remitting fever, and in unfavorable seasons or when a general state of air prevails to aid disease, they produce the yellow fever.

In the fourth and last class we may rank large cities in the neighborhood of flat and marshy lands; where both causes of disease vegetable and animal effluvia in their utmost force, concur to vitiate the air. In such places, sporadic cases of the yellow fever will annually occur; and in bad seasons, the disease will become epidemic. We have yet no large cities in such situations; and in the towns in the southern states, we have hardly an example of the full effect of animal and vegetable effluvia.

If this scale is not correct, sir, I am pursuaded that something like it is founded in fact.

Of the power of the atmosphere of our climate to produce the plague, under certain circumstances, how is it possible to doubt, when we see often a plague among other animals. We have seen a plague among the cats, which unquestionably originated in the country, destroying those animals with uncontrollable fury. This fact is the basis of an argument, which to me is convincing, of the power of causes existing in our climate equal to the production of the plague among men.

We have reason to believe that the bodies of cats are as well fitted to resist disease, as those of men. The powers of life appear to be as complete in one as the other; and that this is the fact, tho we may not be able to prove it like a mathematical problem, yet it is fair to presume it from the analogies of nature in all her productions. If this is admitted, we may fairly assert, that as great a change in the air is necessary to produce an epidemic among cats as among men, that is, the force and activity of the deleterious principle, whatever it may be, must be as great to destroy the power of life in cats as in men.

If then the air or even the diet of cats, is susceptible of a change adequate to the production of a disease which shall sweep them away by thousands, why is it not fair to deduce the result, that a change in the constitution of the air or the nature of diet, may produce the same deleterious effects among the human race? I see no defect in this mode of reasoning, nor can I believe it possible for any man to weaken or elude its force. Indeed it is to me absurd to suppose that causes should exist, in any climate, which may be fatal to one kind of animals; and that in the same climate, causes cannot exist, which may be fatal to the lives of other animals.—In short, sir, it is evident the ancients contemplated, as we now do, this epidemic predisposition to disease, which certainly does exist, tho invisible; and I never think, without

admiration, of the strong and sublime description of it in scripture "The pestilence that walketh in darkness."

LETTER XXI (December 13)

I intended my last letter should have closed the series of facts on which I ground my opinion of the domestic origin of the plague or yellow fever. But you will readily perceive by the desultory manner in which this subject has been treated, that I have paid little attention to the order of narration, and that new facts have occurred to me, since I began to write. Let these circumstances plead my apology for subjoining a few other facts and remarks which are deemed essential to the points under discussion. The importance of the inferences to be deduced from the facts will justify the detail.

Lind, page 3d, remarks, "That there is a disease of a contagious nature, (I should say, according to my definition, *infectious nature*) the product of filth, rags, poverty and polluted air, which subsists always in a greater or less degree, in crouded prisons, and in low, nasty, damp and unventilated habitations, loaded with putrid animal steams, is now well known, and has often been fatally experienced, &c."

The same author observes, "That epidemic distempers begin constantly to rage in the hottest months, which are July, August and September. These complaints, according to the accurate observations of a physician who practised long in Hungary, are altogether the same with those which are epidemic on the coast of Guinea, and the sickly climates of the East and West Indies—viz. malignant fevers of the remitting and intermitting kind and fluxes. The hotter the summer, the more frequent and mortal are the diseases. This was fatally experienced by the Austrians in the unusual sultry summer months of the years 1717 and 1718.—In proportion to the heat, is the pestiferous quality of the marshy exhalations. How farsoever the noxious vapors from unhealthy grounds may spread themselves, it is demonstrable that their malignity decreases in proportion to the distance to which they are diffused.

The cloaths, linen, bedding and utensils of the sick are apt to imbibe and propagate contagion (infection.) The air of the apartments of the sick, when close and unventilated, becomes often so highly tainted, that I have known six attendants infected by it in less than 24 hours." To this cause the author attributes the great mortality often in ships, which are crouded.

These remarks of that most accurate and experienced observer, are of infinite consequence. They amount to *proof* of the doctrine for which I contend, that the *power of propagation*, in the malignant diseases under consideration, is not a *specific, inseparable quality*; but depends on *concentration*, or want of fresh air and cleanliness.

This, Sir, is the point I wish to make clear to every one of my fellow

citizens in the United States. It is the great preliminary object I have in view; for unless this point should be *conceded* or *demonstrated to universal conviction*, all exertions towards avoiding or alleviating this formidable fever, will be of little avail. We must believe, and we must *all* act from the conviction, that *human* efforts are competent to *control* or *mitigate* the yellow fever and if we do not, with unanimity, come to this point, we may as well adopt the Mahometan creed of *fate*, and sit down and die without a movement to avoid the destiny.

That the stupid believers in the Koran, should thus succumb to the ravages of the plague, is not very surprising.—Their conduct is the consequence of their principles. But, in this country, where the mind is unfettered by religious dogmas, and free to range in the field of inquiry, as the eagle to soar in the etherial fluid, it is dastardly and degrading to sit down contented with the prejudices of the oriental world, and believe them truths. The lives of millions of men, and the permanent prosperity of all our great towns, are to depend on a just view of the origin and nature of the yellow fever; and in this country, a free inquiry, after combating innumerable prejudices and mountains of obstacles, will prevail and correct the errors of other nations. The progress of truth is slow, but facts and investigation will attack error by piece-meal, and finally demolish its whole fabrick.

The following letter adds to the mass of evidence already laid before the public, in proof of the opinions advanced in these letters; at the same time, illustrates the progress of truth on this subject; as the writer is a recent convert to those opinions.

The letter is from Dr. Osborn, Physician to the garrison on Governor's Island; who has also had the charge of the sick at the Lazeretto on Bedlow's Island:

"To the Commissioners of the Health-Office of New York"

(The first part of the letter relates solely to some inconveniences experienced for want of a more suitable building for a hospital, and other circumstances not to my purpose. The latter part of the letter is in the following words:)

The liberal supply of bedding and beds, and every other necessary article, with which the commissioners have furnished the institution on Bedlow's Island, has enabled the house to be kept in a state of complete cleanliness. The result of this has been, that notwithstanding the many instances of yellow fever which have come under my care, some of which possessed a malignancy rarely exceeded, there has not been one occurrence in which the disease has been communicated either to attendants or visitors. Nor have any of those who have been engaged in conducting the sick from the different vessels to the hospital, suffered any bad consequences, tho exposed to bedding and cloaths of the diseased, in the state in which they arrive from sea.

It affords me a very particular satisfaction to be enabled to furnish you with these facts; and if from a want of opportunity hitherto, my mind had not been convinced of the real nature of the yellow fever, as to the degree in which it is infectious, I should now have no more hesitation in attending persons laboring under this disease, provided due cleanliness and ventilation was observed, than in any other instance of fever.

I cannot conclude without observing that I have received the most important assistance from Dr. Bailey, by his daily attendance and advice on the most interesting cases, that have come under my care in the hospital; and that to him I owe much of the satisfaction I shall derive, if my exertions for the relief of the sick have been such as to merit your approbation.

I am, Gentlemen,

Your obedient servant,

SAMUEL OSBORN.

Bedlow's Island,
November 1, 1797.

If, sir, the yellow fever is a disease of specific contagion, or even of very high contagion, why does it not exhibit its effects in such places as the Lazeretto on Bedlow's Island? Dr. Bailey is my author for asserting that persons ill with the fever from Philadelphia and from the West Indies, have the summer past, been lodged in the same ward of the hospital, with other diseases. No infection has appeared among those other diseased persons, nor their attendants. That not one solitary case of infection should occur, is evidence that when the air is kept sweet by changes of cloaths, cleanliness and fresh air, *in an atmosphere where the disease was not generated*, and which does not predispose to the disease, the yellow fever exhibits *no infection*.

It is not improbable, but rather probable, that the same number of sick, in a similar building, in one of the *low dirty alleys of this city*, notwithstanding precautions, might have communicated the disease to some of the attendants. In this case, however, the effluvia from the sick would have operated only by aiding the original cause of the disease; that is, by augmenting the vitiation of air in that part of the city. It is the *combined force* of these causes which is to be dreaded. It is rather the *contagious* influence of the air, already vitiated by external causes, united with and assisted by the poisonous effluvia from the sick, which last I call *infection*. I believe either of these, separate from the other, *may* be so concentrated in a particular place or person, as to give the disease to a well person; but I firmly believe human power may so dissipate them, as to render them *generally harmless*.

The following letter from Mr. Banker, late Treasurer of the State had it been received in time, would have been subjoined to the account of Mr.

Phenix's illness. It corroborates the doctrine I maintain respecting the operation of a *vitiated air* on the human body, at all times, and in all places, without regard to climate.

New York, December 28th, 1795

Dear Sir,

Agreeable to your request of this morning, I sit down to commit to writing the circumstances that attended a fever I had in August 1788. In about a month preceding that time, I had been receiving a large quantity of dirty defaced state paper bills of credit, to the number of above one hundred thousand bills. For safe keeping they were locked up in a very large chest, in my bed room, which was a small room and had no air passing thro it. As the quantity of bills increased, I found the keeping them locked close, occasion a very offensive smell, and I therefore threw open the chest at night; in the day time it would have been unsafe.

I had for about a fortnight endeavored thus to air the chest, when I was seized with a violent fever, and a distressing pain across my forehead. Dr. Cochran was consulted, and by his advice I immediately removed to an upper room (the one I occupied being on a lower floor, and not a dry room). I followed his prescriptions for about a week; and he then had me blistered between the shoulders. I obtained relief and after a fortnight's confinement I quit my room.

I am sir,

Your humble servant,

G. BANKER.

Dr. Bailey:

It is remarkable that a fever, (which I may call yellow fever as it was attended with that prominent symptom of the late fever in Philadelphia, a severe pain *across the forehead*, or just above the eyes) was generated by the foul air of a room, contaminated by a large quantity of dirty bills, inclosed in a chest and fermenting in hot weather. And this too in August 1788—five years before the fever appeared in Philadelphia, and before this country had witnessed the smallest epidemic constitution of atmosphere to aid in the production of the disease.

Surely, Dr. Currie, such acts must remove from your mind and the minds of your candid fellow citizens, every particle of doubt about the possibility and probability of the domestic origin of yellow fever; and I am satisfied, that this *is* the fact, the future safety, happiness and prosperity of Philadelphia, and all other large cities, are suspended on a general belief of the fact.

LETTER XXII (December 14)

I should be afraid, Sir, to add this one Letter to what I have before written on the doctrine of epidemics, had I not been assured, from various quarters, of the very favorable reception of my remarks by many of the faculty in this city, by the commissioners of health and many respectable citizens. This circumstance, together with my conviction of the extreme importance of this subject to the future health and happiness of our new Republic, has induced me to subjoin an account of that terrible pestilence, the ulcerous sore throat, which ravaged our country in 1735 and 1736. The account is from Dr. Belknap's history of New Hampshire, vol. 2 page 118, and is in the following words:

"About this time, the country was visited with a new epidemic disease, which obtained the name of the *throat distemper*. The general description of it is a swelled throat, with white or ash colored specks, an efflorescence on the skin, great debility of the whole system, and a strong tendency to putridity. Its first appearance was in May 1735, at Kingston in New Hampshire, an inland town, situated on a low plain. The first person seized, was a child, who died in three days. About a week after, in another family, at the distance of four miles, 3 children were successively attacked, who also died on the third day. It continued spreading gradually in that township, thro the summer, and of the first forty who had it, none recovered. In August it began to make its appearance in Exeter, six miles north eastward; and in September, at Boston,* 50 miles southward, tho it was October before it reached Chester, the nearest settlement on the West of Kingston. It continued its ravages thro the succeeding winter and Spring, and did not disappear till the end of the next summer.

* On its first appearance in Boston, it was supposed to be nothing more than a common cold; but when the report of the mortality was received, and a young man from Exeter, whose brother had died of it, was seized (October 1735) the house was shut and guarded, and a general alarm spread thro the neighboring towns and colonies. Upon his death, no infection was observed in that house or neighborhood; but the distemper appeared in other places, which had no communication with the sick. The physicians did not take the infection nor convey it to their families, nor their other patients. It was therefore concluded, that it was not like the small pox, or the plague, communicable by infection, from the sick or from clothes; and the physicians, having by desire of the selectmen, held a consultation, published their opinion; that it proceeded entirely from 'some occult quality in the air.'

Weekly News Letter, April 29, 1736.

Dr. Douglas computes the number of persons who had the distemper in Boston at 4000; of whom 114 died, which is one in 35. The whole number of inhabitants at that time was estimated at 16,000.

"The most who died of this pestilence were children; and the distress which it occasioned, was heightened to the most poignant degree. From three to six children were lost out of some families; several buried four in a day, and many lost their all. In some towns, one in three, and in others one in four of the sick were carried off. In the parish of Hampton Falls it raged most violently. Twenty families buried all their children. Twenty seven persons were lost out of five families; and more than one sixth part of the inhabitants of that place died within thirteen months. In the whole province, not less than one thousand persons, of whom above nine hundred were under 20 years of age, fell victims to this raging distemper.

"Since the settlement of this country, such a mortality had not been known. It was observed, that the distemper proved most fatal, when plentiful evacuations, particularly bleeding, were used; a great prostration of strength being an invariable symptom. The summer of 1735, when the sickness began, was unusually wet and cold, and the easterly wind greatly prevailed. But it was acknowledged to be, not 'a creature of the seasons'; as it raged thro every part of the year. Its extent is said to have been 'from Pemaquid to Carolina'; but with what virulence it raged, or in what measure it proved fatal, to the southward on New England, does not appear.

"The same distemper has made its appearance at various times since. In 1754 and 1755, it produced a great mortality in several parts of New Hampshire, and the neighboring parts of Massachusetts. Since that time it has either put on a milder form, or physicians have become better acquainted with it. The last time of its general spreading was in 1784, 5, 6, and 7. It was first seen at Sanford in the county of York; and thence diffused itself, very slowly thro most of the towns of New England; but its virulence, and the mortality which it caused, were comparatively inconsiderable. Its remote, or predisposing cause, is one of those mysteries in nature, which baffle human inquiry."

I regret, Sir, that I have not an equally correct history of that formidable disease, in other parts of this country. It was a general and terrible pestilence; and numbers of old people, now living, can doubtless state many useful facts about its first appearance, progress and symptoms. The distemper is a distinct disease from that class to which the yellow fever belongs; but it was another species of *plague* and exhibited a number of phenomena which correspond with those of the plague, and which serve to illustrate the doctrine of epidemics.

First. It was, says our faithful historian, a *new* epidemic in this country. It might have occurred before in particular cases; but never spread as an epidemic and mortal pestilence.—The *novelty* of the disease therefore is no objection to its existence, nor to its domestic origin: Another fact to overthrow the grounds of your doctrines.

2. It was generated in *our* country. It appeared in an inland town of New Hampshire, on a low plain, in a remarkably cold and wet season.

3. It was *highly infectious*; but was not spread *solely* by infection. On the contrary, it originated in a *contagious* influence of the atmosphere, in various distant places. A decisive proof of the doctrine I am maintaining; of an epidemic constitution of the air.

4. This disease exhibited, in a most striking manner, the phenomena before mentioned of the plague in Italy, and in the United States—That is, passing by, or so to speak, *leaping over* towns near or adjacent to the infected places, and breaking forth in distant towns. Thus the sore throat appeared at Kingston in May; at Exeter, six miles northeast, in August—In Boston, fifty miles southward in September—and in Chester, only six miles westward from the original source of infection, not till October. And it is noted that in Boston it was not communicated by infection from another town. It had then *passed by* the intermediate towns, which are numerous and large, between Kingston and Boston. I say passed by; not that I mean a contagion had *moved* from New Hampshire to Boston; I use the words in conformity to Dr. Mead's idea, that a disease is carried from place to place by infection.

But this is not my own opinion. I believe this state of air, which I call *contagion*, to originate from *general causes* which are *unknown*, aided by *local causes*—That is, a general predisposing state of the atmosphere, exists at a particular period—and whenever local causes unite their influence with it, a disease will appear where those local causes operate. This may be in various distant places at the same time.

5. A circumstance related of this disease is remarkably analogous to one observed in the plague; viz. that it was *most mortal on its first appearance*. Of the *first forty* children affected in Kingston, *not one recovered*. The plague was probably never so fatal. It afterwards assumed a milder form; and in Boston but a small proportion of the diseased died. On the whole, the mortality equalled that of the worst forms of the plague. In fifteen towns, in New Hampshire, which might contain on an average one thousand or twelve hundred souls each, more than a thousand persons died, mostly children.

I have one idea to suggest on the subject of epidemics, which may be useful in illustrating the doctrine of a contagious constitution of atmosphere. This is, that men of leisure and who have large libraries, should examine how far mortal epidemic diseases have occurred about the *same* time, in different *countries*. I cannot at present examine this point. On recollection, it occurs to my mind from reading that some remarkable coincidences of this kind are observable. The general pestilence in the days of Justinian, and that in 1346 to 8, have been noticed.

The time of the terrible sore throat just described corresponds precisely with the most dreadful plague Egypt has known this century, viz. 1736. It was said that 10,000 died in Cairo in a day—that is probably an exaggeration.

The yellow fever appeared in Charleston in 1741 and about that time in New York. This might all be the same period of pestilential constitution in the air. The general plague in the Levant, described by Russel, appeared in 1758, and lasted till 1765—a period of seven years. And with this period corresponds the appearance of the yellow fever again in the southern States of America; viz. in 1761. The present period of yellow fever in the United States has lasted upwards of four years, and in this period, the same disease has raged with such unusual violence in the West Indies, as to be supposed a *new* disease and *imported* from Africa. *See Chisholm.*

These phenomena result from some established laws of the universe—To attribute such extensive effects to *infection*, is like ascribing the flowing of tides to a puff of air from a handbellows.

The subject is as curious as it is useful, and I wish I had time and the means to pursue it. But enough has been written to convince candid impartial inquirers after truth: It appears to me that, those who will not believe the facts here related and their necessary consequences, would not believe "tho one should rise from the dead."

The application of the principles here explained, the most important part of my subject, remains alone to be considered.

LETTER XXIII (December 15)

The length to which this discussion has already been extended, will cause me to be very brief in stating my inferences from the facts which have been detailed.

If the doctrines laid down in the preceding letters are just, we are conducted to the following causes, as the sources of the yellow fever.—First, the direct effluvia from a diseased person—secondly, the vitiation of the air in particular districts of country, from the animal or vegetable substances—thirdly, some occult quality of the atmosphere, which prevails occasionally over a country, and aids in spreading the disease.

On these principles, which I have reason to be persuaded are just, I shall, for my *own* purposes, raise definitions of *infection* and *contagion.*

The first source of the disease mentioned above, or direct effluvia from the sick, I shall call *infection.*

The second source, or the effluvia from marshes and cities vitiating the adjacent air, I shall call *local contagion.*

The third source or auxiliary cause, the general predisposing influence of the atmosphere, occasioned by seasons or unknown causes, I shall call *general contagion.*

The first cause, infection, it is agreed by the best writers, does not operate beyond the distance of a few feet. Russel and Chisholm both agree in this.

The plague or yellow fever is rarely communicated at the distance of *ten* feet from the sick; and the sick must be shamefully neglected, to render the effluvia from their bodies or garments *dangerous*. If kept clean, the effluvia are not powerful and active enough to communicate the disease at all—Hence the inference that the fears of people are mostly ill-founded. The danger of infection depends more on the nurses and attendants, than on the specific nature of the disease.

Another inference from the fact is, that *infection* alone will not and cannot render the yellow fever or plague *epidemic*. It is absurd to suppose that a disease propagated by effluvia from the sick alone, can be spread over whole towns, in a few days or even weeks. Epidemics are propagated mostly by *contagion*, local or general.

A third inference from the fact is, that the yellow fever or plague never can spread by infection among the scattered inhabitants of country towns. It must be limited to populous towns, where the houses are contiguous, or nearly so; or to some clusters of dwellings in the country.

A fourth inference is, that the yellow fever when it appears in a particular city, is confined to the air of that city, and will not readily propagate in another place. Hence it is useless to cut off communication with infected towns. The multiplied proofs of this quality of the disease leave no room to question the fact.*

On the head of *general* contagion, I shall make but one or two remarks. It is a thing wholly beyond our power; and known only by its effects. All we have to do, is, when we observe this contagion to exist, and diseases to spread with uncommon rapidity, to bend all our attention to lessen its effects by destroying all *local* causes which may aid its source, and by adopting that mode of living which may weaken its action upon the system.

It is on the second cause of malignant disease before mentioned viz. *local contagion*, that human power and policy must be exerted.—And here I must notice a most consoling fact, that this most formidable disease, the plague or yellow fever, is rarely or *never* generated by *general contagion*, which is beyond human control; but is always generated by *local contagion*, which arises from causes which men can destroy or avoid.

This is a most material fact, and renders the fever, *when understood*, a less terrible distemper than the putrid sore throat. We see the fatal sore throat arise in any place, we know not how. We can neither see its causes nor escape its effects by removal. It is generated and propagated not only by infection, but by a contagious influence which is invisible and uncontrollable

* It is mentioned that a person from Red Bank caught the fever the summer past in Philadelphia, went home and died; and that most or all of the family, being eight or nine, caught the disease from him and died also. But I will venture to assert, that either there was gross mismanagement of the sick; or the house is in a situation where a powerful *local contagion* assisted the progress of the fever.

—It rages in all seasons and in all situations, with slight differences only in its malignancy.

Very different is the origin of the plague. This disease always begins in a particular season, rages a few months and abates, or is extinguished. It is limited to places where the air is corrupted by causes which are usually in human power—it originates in damp, hot, stagnant air; and never spreads where the air is not vitiated by local causes. Nature has set bounds to its ravages; for altho general contagion may at times favor its *propagation*, it appears never to be equal to its *original production*.

From this consideration we are led by duty and by interest to investigate the *causes of local contagion*, and to devise the means of removing them, or of reducing their effects, in such a degree as to render them harmless.

Malignant autumnal fevers prevail most in cities and about marshes.

Marshes are not to be dreaded while covered with woods. In this state, the mass of decayed and decaying vegetables and water, lie dead and inactive. But when the shade of the trees is removed and the foul substances laid open to the sun, the noxious parts are separated and put in motion.* Hence the *first* cleaning of lands on wet and swampy ground, is followed by fevers, usually intermittents. Of this we have a multitude of instances in the United States. But, in all situations, where land can be thoroughly dried by the sun, this evil is temporary.

It is near large ponds and swamps, abounding with grass and weeds, and which cannot be drained, that the danger of fevers is permanent. Men that plant their dwellings near them, must expect to be frequently visited with intermitting and remitting fevers. It is the operation of a law of nature which no human art can control. The contagion is more active, and extends to a greater distance, in one season than in another.

The precise degree of danger therefore can be known only by the events of each autumn. Dr. Buel's observations in Sheffield afford the fairest ground for an opinion as to the distance which local contagion extends from its source, viz. one mile and a half; this being the limit of the epidemic fever about the Sheffield ponds.

The Atlantic states of America, with respect to their soil and healthfulness, will admit of a division at the river Hudson. Northward and Eastward, of that river, the coast and inland territory, consists generally of high and dry lands; and intermitting fevers are rarely known. A few ponds, and marshes made by creeks from the ocean, may be excepted, and about these we observe

* It may be added as another reason why the clearing of wet lands causes fever, that the noxious particles, which are known to be absorbed by growing vegetables, as a part of their nutriment, are left, by the destruction of the trees, to float in the air; thus vitiating the first and most essential principle of life. The same effluvia which are the best manure and food for plants, are the most fatal poison to the human body.

fevers. But on the greater portion of the territory east of the Hudson, a case of ague and fever is a rare occurrence, except along the borders of that river. In this state of New York, slight intermittents are common, and here begins the coast to subside into a flat country.

In the lower parts of New Jersey, the coast takes the level aspect, which runs thro the territory of the United States to the southward; and from this point autumnal fevers become annual complaints, differing in violence in different seasons.

A level country even when dry is less healthy than a hilly one; whether owing to a stagnant air or bad water, or both. But vast tracts of the low country southward of the Hudson, are covered with water from the ocean; and marshes made by the tides can never be corrected but by dykes. Those parts of the southern and middle states therefore which are near permanent marshes, are doomed to an annual return of fever.

I lay it down as a principle deducible from facts, that wherever an *ordinary* season will produce intermittents, and an *unhealthy* season, the remitting fever, in an *open country*; in a populous *city* built in that country, the addition of artificial causes of an impure air, to the natural contagion of the country, will, in a healthy season, produce remitting fever, and in unfavorable seasons, the yellow fever or plague.

Of this, Egypt affords an example, and if I mistake not, some parts of the southern states, whose situation furnishes more of the noxious principles of disease, than even Egypt itself.

Volney tells us that the Egyptians are subject to bilious complaints, troubled with nausea, and are of a sallow complexion. Why? Because they live on a large plain, covered, a part of the year, with water, and afterwards exposed to a burning sun. There is not an instance on earth where men, living in a hilly country, are troubled with annual epidemics of the same kind. There is not an instance in the middle latitudes where men, living on low grounds, in the neighborhood of marshes, escape those epidemics.

Now in Egypt, where bilious diseases are the common diseases of the country, in ordinary seasons; a bad season converts those diseases into the plague, and the large cities are ravaged. So in the southern states, bilious fevers are the annual complaints of the low country, and in bad seasons, the large towns are afflicted with the plague, or a malignant fever nearly as mortal.

The result of these observations, is, that the large towns, if built on the low flat country of the middle and southern states, must be laid out and constructed on a plan very different from the cities now built in the United States, or it is their doom to be *frequently* laid waste with the plague, and *annually* infested with malignant fevers. On the certainty of these events, I am willing to hazard my reputation.

I write and publish my opinions on this subject with some apprehensions

of exciting the clamour of men who have property in situations evidently unhealthy. But on a subject so interesting to my country, involving the fate of millions yet unborn, to be silent would be criminal in a man who believes in these principles.

Yes, sir, I believe it my duty, as a citizen of a new country, whose cities are not yet founded or not grown to a size incapable of amendment, to address my countrymen on this subject in frank and explicit language—The whole flat country from this city to St. Mary's and even to the gulf of Mexico, is formed by nature to be *unhealthy*; and *more* unhealthy than any tract of the same extent in Europe, and so far as we know, in Africa. The lands adjacent to the Nile in Egypt and Abyssinia, are naturally *less unhealthy* than the Atlantic coast of America for twelve or fourteen hundred miles.—Europe has no such extensive tract of level country. The marshes in Hungary are the principal, and those produce annual fevers.

The territory of Rome, the ancient Campania, is a level of 30 or 40 miles in extent. To the southward lie the paludes pomptinae, pontine marshes; which, when ancient Rome was in its splendor, caused a pestilence every few years. The country is now almost depopulated; & the Appian way, which was carried over that marsh, is abandoned on account of the unhealthiness of the air, and another road to Naples is carried *round* this contagious spot.

In no other country, do we find such immense tracts of low swampy land as in North and South America.—But in South America, the low lands I refer to, are within the tropics, subject to perpetual heat. This renders the country very insalubrious to *strangers*; but it has been observed, that the *natives* of such climates are fitted to resist the contagion. Therefore even the unhealthy countries of the tropical latitudes are less to be dreaded by them, in general, than the low and marshy grounds in the middle latitudes, which have alternate heat and cold.

We live in those latitudes, Dr. Currie; the very climate most exposed to violent, malignant bilious fevers. In winter, we may be said to be inhabitants of Lapland; in summer, of Mexico or the West Indies. If we do not remove to the polar circles or to the tropics, yet the revolutions of the seasons *bring their climates to us*; and we annually run a similar risk with the Europeans, who pass from the northern to the southern latitudes, to *perish in multitudes*.

It is a most unquestionable fact that the northern states of America from New York to Maine, are in a position on the globe, as exposed to the plague as the cities of Marseilles, Naples, Rome and Constantinople, and the middle and southern states have a position corresponding with the latitude of Syria, the Barbary coast and Egypt; that portion of the earth which is *most frequently* ravaged with pestilential diseases.

LETTER XXIV (December 18)

If then we live in a climate in which the human body, from alternate heat and cold, is most irritable, and most subject to malignant autumnal distempers; and if a part of our country is peculiarly adapted to the production of such diseases; the most serious of all questions arises; what shall we do to prevent a frequent return of such calamities?

To me, sir, the path is extremely plain. Our climate we cannot change—much of our country cannot be raised into hills, nor drained of its stagnant waters—the laws of nature we are unable to control in their operation,—but *our* duty is plain. Men must not plant their habitations near marshy grounds—the mode of building cities *must* be totally changed—and so must the usual habits of our citizens.

Throughout the whole Atlantic territory, on the low lands, it is more essential that large towns should be purposely constructed for health—Great cities are usually founded on commerce, and commerce requires the access of navigable water. Hence such towns are commonly near the sea shore or on the borders of rivers.—But if they are near low marshy grounds, they cannot be healthful. High and dry positions, with rocky or gravelly earth, are the proper places for populous cities, on account of fresh air and good water.

But in any situation, our cities are too crouded for health or comfort. Lots are too small—too many people are crouded on to a little space of ground—A family to every lot of 25 or 30 feet by 80 or 100, cannot fail to generate too much filth, and to vitiate the air in too great a degree, for the health of the citizens. In every large town in the United States, however remote from marshes, and however healthy its position, the effects of crouded population are obvious, every autumn, in the sickness and health of *children*. Multitudes and multitudes of lives are annually sacrificed, in all cities to the avarice of the original proprietors of lots. The little narrow dirty houses, kitchens and yards surrounded with high fences, excluding air and vegetation; all that can dissipate or absorb the noxious exhalations; all that can purify the atmosphere and refresh the exhausted frame of a human being, panting beneath a sultry sun—every thing in our cities is contrived to ***waste the powers of life, and shorten its duration.***

Men, in this respect, are infinitely less sagacious than *irrational* animals. Instinct guides the beasts of the fields to the most proper habitations—and they never reside where they are *annoyed*. But man, with all his boasted reason, sees the effect of his folly, and hundreds and thousands of his fellow citizens falling victims to his own neglect, his mistakes or his sordid principles; he heaves a sigh in August and September, as he views the sable hearse, conveying his friends in scores to their graves; in November he shrugs his shoulders and says, *it is all over*; runs to the Circus, the theater and the card room; laughs away the winter's evening with his jovial companions, some of

whom are destined, the next season, to fall a sacrifice to the same folly and neglect, and to fill new ranges of graves by the side of the victims of the preceding year.

In the United States, every thing that has been done hitherto in the construction of cities is in imitation of the old European and African mode, and of course is *wrong*.

The ancient construction of London cost that city nearly *two hundred thousand* lives in one century; and Cairo and Constantinople probably lose more than that number, every half century.—I firmly believe, and my belief is founded on the uniform operation of established laws of nature, that a *perseverance in our present mode of building cities, will doom them all to the same fate*. I believe it, because I see no possible reason why pestilential diseases should not be as frequent and as fatal in America as in the world, under the operation of similar local causes.

If a rigid police can be uniformly observed; every possible nuisance be removed by shovels, brooms and water, cities in healthy positions will escape the regular and constant return of malignant epidemics, as New York has done the summer past.—Multitudes of lives may be saved, and the loss of business prevented by these means; and in cities already built, cleansing, washing and purifying, are the guardian angels of public health.

But I maintain that this is not exterminating the root of the evil. Cities *may* be built so as to unite all the utility of a town, with the salubrity and pleasures of the country, and in the new world, where men are as free to act as to think, it will be dishonorable not to invent and execute a plan for these purposes.

Were I called upon for a plan of a city, which should combine in it the advantages of town and country, I should suggest the following outline:

1. The position to be on the shore of the sea or the bank of a river, gently ascending with an angle of at fifteen or twenty degrees; which would form a declivity for a rapid decline of water to wash the city; at the same time would not prevent the draft of heavy loads on the river or ocean.

2. The wharves on the water should be extended beyond low water mark, that no part of docks should be left bare by the recess of the tide. All the solid front of the wharves should be on a line; that no eddies might be formed; and the water might flow in a passing current. This would carry off substances thrown into the water, and contribute to keep it pure by motion and change. Should it be necessary to extend wharfing in one place other than in another, it should be in the form of a bridge on piers.

3. The warehouses on the wharves should not be jumbled together in a chaos; one projecting in front of another: but built in a strait range at a suitable distance from the water; and an alley should pass between them for the admission of fresh air on every side.—No wet cellars should be permitted.

4. The streets should be strait, intersecting each other at right angles, and forming the city into squares of at least 500 feet on a side.

5. The lots should be at least 60 feet wide, and 250 feet long. No more than 35 or at most 40 feet of breadth of the lot to be ever covered with buildings, so that a space of 20 or 25 feet should intervene between the houses. Thus space would leave a cartway, given free admission to air, and reduce the risk of fire 75 per cent.

6. In the rear of the front building, out-houses might cover 100 feet of the length of the lot. The remaining 150 feet should be laid out to the fancy of the owner in a garden—the police interfering no farther than to require by law that some kind of trees or vegetables should occupy the space—vegetation being the natural purifier of the air.

7. Directly thro the center of each square, should run a narrow street of about 25 feet taken out of the rear of the lots, which street or alley should never be built on, but would admit a passage for citizens thro the squares; and what would be still more necessary and useful, would admit dust carts to take up all the filth of the houses, from the rear, and prevent that intolerable nuisance, the depositing of offensive material in the streets. The little flower gardens and shady lawns in these squares, would preserve the air pure, fresh and cool in the summer.—they would occupy many persons of delicate health, thus contributing to their comfort, diverting their minds, and in many cases, restoring them to health. Here also children would find room to gambol, without endangering their lives or annoying passengers in the streets; and here the young of both sexes would acquire a taste for gardening, for botany, and the delightful amusement of studying the works of nature.

8. The streets should be one hundred feet in breadth, and planted with three rows of trees. One row on each side next the foot walks, and a row in the center. The foot walks to be 15 feet wide. The trees next to the walks would then be 15 feet from the houses; and a space between the center row and each side row, of 35 feet—The trees might be so pruned, as to prevent their injuring the buildings and their distance would prevent their interfering with fire engines. These trees would be leafless in winter, when the sun is acceptable; and in summer would sweeten, purify and cool the air. The citizens at all times in the day, would walk in a refreshing shade,—the center row of trees would furnish shade for horses.

9. No city should be raised on level earth. If a natural position cannot be found, with a general declivity to the water, the streets should be thrown into artifical elevations of at least 3 feet in every hundred—five feet in the hundred would be better. This would give celerity to the water falling in showers, and wonderfully assist in removing filthy substances from the streets; for after all human efforts in sweeping, much offensive matter will remain, which water alone will reach and carry off.

10. No pains should be spared to supply a city with fresh running water.

Here let me expostulate with my fellow citizens, whose health, whose lives, whose happiness and prosperity are concerned. *Pure air and pure water are infallible antidotes* to the plague. Where these exist, it is not in the power of man to spread that formidable disease. I defy the man who will produce one solitary instance of a plague or yellow fever, which has been propagated, and become epidemic in any climate, unless the air has first been highly vitiated. And the vitiation or local contagion, is ninety nine times out of a hundred, either *wholly occasioned* or *greatly augmented* by the negligence or improvidence of men. In a hilly dry country, if towns are ever infested with the yellow fever, it is the *sole fault* of the inhabitants.*

It is a point of infinite importance that citizens should not depend on water from pumps in the city. In a few years, the subterranean water becomes impregnated with the noxious particles from vaults.—and this evil continues to increase with the age of the city. One of two remedies are to be provided—either the city must be supplied with fresh water by pipes from a distant source; or the vaults must be so formed as to be capable of being opened in winter and cleansed. But the last method tho useful and practiced in some European cities, would be ineffectual.

The back houses in a city are, in many respects, a terrible nuisance. If a sewer could be carried four or five feet under ground, and every such house be set over it, a stream of water passing thro it from a distant source, would be an excellent means of cleansing a city from this source of disease. But such streams of water are rarely to be obtained.

Another expedient suggests itself for the same purpose. Let a sewer of three feet wide and well paved, be run in a strait line under the rear of the lots, and all back houses set over it. In suitable places, let channels be made to turn the water from the streets into the sewers, in long or violent showers, when the water is not wanted to wash the streets. In this manner, the sewers might be washed perfectly clean, a number of time every summer, and the citizens preserved from their poisonous exhalations.

On a model of this kind, I conceive cities should be constructed in the *most healthy* situations; for a crouded population, in any place on earth, will lessen the salubrity of the air. Close compact cities, in any quarter of the globe, are the graves of men. All the great cities of Europe require annually some thousands of strangers to supply their waste of population. Yet there is no necessity for men to croud together in such a compact form. There is land enough on earth to suffer any extension of cities, and it makes little difference, in the first instance, whether a lot contains one hundred feet of

* If any exception ought to be made to this remark, it must be when there exists a general contagion of extraordinary activity—like that which probably prevailed in the great plague of 1348. Such a state of air rarely exists, and we have not facts enough respecting it to lead to positive conclusions, as to its effects in places where there is no *local* contagion.

land or half an acre. Nor would a less dense population be any inconvenience to men in business—They might be obliged to walk further on some occasions; but in the cool shade of my proposed city, this would be a pleasure rather than a toil.

It is wrong, it is criminal, for legislatures to permit such crouded population. It is a nuisance, not only to cities but to the public. It is a truth and I will proclaim it, that numbers of lives are sacrificed almost every year, among worthy country gentleman, who have business in our large cities.

They come to town in the hot season, when no uncommon disease prevails among the citizens who are innurred to the air—they come without suspicions —They are seized with fever and die.—the air of the low grounds in our cities, even in healthy seasons, is often poison to people from the country, and gives them a fever, when no epidemic is visible among citizens. I know the fact, and it is a serious calamity, especially to seamen. The low grounds in Providence, New York, Baltimore &c. are the greatest nuisances now existing in the United States. The building of Water street and Front street in New York, it is believed by good judges, has cost this city a thousand lives in five or six years. I say nothing of Philadelphia; for its position and the alterations in the original plan of the city have doomed it to calamity. The citizens will not believe the evil to be among themselves and therefore must be left to their fate.—If remitting fevers *every* year, and yellow fever *often* will not convince men that something is wrong in *their* city, it is in vain to reason with them. Of one thing I am as certain as I am of my existence, that if all the earth in New York on which Water and Front streets are built, could be sunk 30 feet under water without loss of lives, and the proprietors indemnified by the citizens, it would be the greatest blessing which heaven could in mercy bestow on the city and the state. I believe also, that if all the cross streets and the back houses in Philadelphia could be leveled with the earth, and the ground converted in flower gardens and grass plots the citizens would, in twenty years, celebrate the anniversary of their destruction, with as much fervor as the republicans in France celebrate the demolition of the Bastile.

It is not possible, I speak it with zeal and confidence; it is not possible, under the operation of the *present* laws of nature, for men to be healthy in many of our cities, during the heat of summer. That open champaign country, which regularly produces intermitting and remitting fevers, will, when planted with populous cities, often and perhaps annually, produce the plague. Of that country, there is an extent in the United States, of more than one thousand miles in length, and from 40 to 60 in breadth.

All the shore of the Atlantic from the Hudson northeastward will admit of healthy cities. If the commercial towns on that portion of America were constructed on the foregoing plan, I would answer for it, that they would never be ravaged with yellow fever. Individual cases might occur; but the

disease could not, without a miracle, become epidemic. But as our towns are now built they will at times be partially affected. In *ordinary* seasons and with a vigilant police they will escape, with the yearly loss of only ten, fifteen or perhaps fifty citizens, the unfortunate victims of the negligence, the folly, and the crouded population of cities. In very *unfavorable seasons*, the number of victims will be *increased—such is the fate of our northern cities.*

But the destiny of cities on the southern Atlantic shore, is to be more severe.

The period of general contagion may subside, and intervals of more general public health, may be expected. The most unhealthy parts of the earth enjoy such intervals, when the rage of malignant complaints is suspended. But the melancholy periods of epidemics will often recur—and as the plague, in all its shapes, is the offspring of *local causes*, mankind, wherever those causes exist, are destined to be afflicted.

Away then with crouded cities—the 30 feet lots and alleys—the artificial reservoirs of filth—the hot beds of atmospheric poisons. Such are our cities—they are great prisons, built with immense labor to breed a jail fever on the largest scale and hurry mankind prematurely to the grave.

P. S. In the Census of 1791, taken by order of Congress, we have obtained grounds for a general estimate of the comparative longevity of the inhabitants in the Northern and Southern states. From Pennsylvania northward, exclusive of Vermont, the free white males *above* 16 years of age were 472,000, excluding fractions; *under* 16, the number was 443,000—difference of 29,000.

From Delaware southward, exclusive of South Carolina, (the Census of this state and of Vermont not being before me) the number of free white males *above* 16 years of age, was 276,000; *under* 16 years of age, the number was 288,000. Difference 12,000. Were the longevity of the southern people equal to that of the northern, instead of a majority of 12,000 males *under* 16 years of age, the southern district ought to have had nearly 18,000 males, majority above 16. Had the females been included, the numbers above would have been nearly doubled, but their relative proportion would not be materially altered.

Now it is well known the mountainous parts of the Southern states are very healthy, as they are in all countries. The difference therefore in the foregoing estimate must be occasioned by the insalubrity of the flat country, bordering on the Atlantic, which may be estimated as containing about half the population at the time of the Census. This circumstance would vary the result very much, as to the relative healthiness of the northern states, and that *low country*. But the fact as stated above is very important; and demonstrates the principles I have advanced, with respect to the salubrity of a dry hilly country, and the insalubrity of a low, flat, marshy country.

Including the females which are nearly equal to the males, the difference

will stand thus.—Majority of inhabitants *above*, 16 years of age, in the Northern states, Pennsylvania included, 58,000. In the Southern states, majority *under* 16, 24,000; exclusive of Vermont, South Carolina and the States west of Allegany—The whole difference of longevity then is nearly 18,000 plus to 18,000 plus 24,000 equals 60,000; or very nearly one ninth of all the white inhabitants in the Southern States specified.—What a prodigious sacrifice of human life—and all attributable to the flat marshy territory on the Atlantic, between the Delaware and St. Mary's. Supposing the whole people of a country to be changed once in 30 years—then at least *sixty thousand persons* fall premature victims to the diseases of the country, in every period of 30 years—all which, Dr. Currie, is the effect of *vegetable* effluvia in *our climate*.

LETTER XXV—AND LAST. (December 20)

There is an extreme repugnance in many persons who reside or have real estates, in our commercial cities, to admit the doctrine of the domestic origin of the malignant fever, which has prevailed in them, for a number of years past; because, they say "it will injure the reputation of those cities."

To me, this reason is not satisfactory, nor can it be well founded. If a malignant fever does, in fact, prevail frequently in a town, it makes little difference in the reputation of the place, whether the disease is generated in that place, or imported from other countries. Country people and foreigners do not concern themselves, where a distemper originates, but whether it exists; and during its prevalence, they will not visit the place.

That the certainty of the domestic origin of the yellow fever, in particular towns, and of its frequent recurrence, would prevent many persons from purchasing and building in those places, may be true; but places of business will never be abandoned. This however is not the point. If the disease is really generated in our country, and in a particular city, the facts will ere long be ascertained to the conviction of every one; and the longer that the conviction is postponed the worse will be the evil, and the more difficult to be eradicated. Should therefore future proofs compel a universal assent to the truth, and recent facts and the laws of nature indicate that this will be the final event, perseverance in error will have multiplied the obstacles that oppose a remedy and rendered an acknowlegement of the truth, more prejudicial to private interest.

But do facts warrant the apprehension that an acknowlegement of the domestic origin of the fever will injure the owner of real estates? It is generally admitted in New York, that this fever has its origin in the city; yet no inconvenience results from the belief. Property in general is as high, and business as flourishing, since this opinion has prevailed, as before; except so far as political events have been unfavorable to trade. While an

epidemic prevails, business suffers; but no sooner does cold weather restrain its progress than people croud to the city and business revives.

In Boston and Newbury Port, it is agreed, the yellow fever was of domestic origin; yet those towns are not deserted nor materially injured.

Baltimore is more directly in point. The yellow fever, in that city, has prevailed often, and with great mortality; yet the place is flourishing, under a universal belief that the disease has always originated in that city. The same is true of Norfolk and Charleston, where the same disease, in different forms, is an annual autumnal complaint.

I do not believe therefore that the belief and acknowledgement of the domestic origin of the yellow fever will materially injure the property of the citizens, in places where it prevails. Men will still throng our cities, and multitudes, impelled by interest or necessity, will maintain their ground and meet danger, in hopes of escaping themselves, whatever be the fate of others. The certainty of the loss of vessels at sea every year, has no effect in restraining men from encountering the danger; and the prospect of falling in battle does not prevent the enlisting of soldiers.

But whatever may be the wishes or opinions of interested men, on this point, physicians and legislators ought to know about the truth, if discoverable, and found their measures on the broad basis of public happiness. The question ought not to turn on the interest of the proprietors of lots, houses and wharves; but on the *security* of the *lives* and *health* of the citizens. If any thing is wrong in the construction of cities, or in the regulations of the police, faithful legislation and good men will search out the errors and correct them.

I hold it to be a duty for the legislators of the several American states to prohibit the division of lands in a city into small lots. It is as clear as any proposition in Euclid that thousands of lives are annually sacrificed to *crouded population*. It is not too much to assert that from a *twentieth* to a *tenth* of all children under two years old, in the healthiest city in the United States, fall victims annually to this single circumstance, the *destruction* of *respirable air by means of close, compact population*. In some of the southern towns, it has been said, that *no child was ever reared*; so fatal is the air to their tender frames. This is probably an exaggeration, but so much is true, that in many places, but few children survive the insalubrity of the climate.

In Philadelphia and New York, this evil is less observable; but even in these places, what a waste of human life is to be seen, every autumn! Not a day passes, for months, in which infants are not carried to the grave—Not from any defeat in their constitutions, or any want of care in parents or physicians; but for the want of a pure, wholesome air. The rich remove from town, as from a seat of infection, and preserve the lives of their children; but the poor, whose lot it is to labor daily for bread, must remain in their crouded hovels, where their infants pant for fresh air and die.

If these ideas are well founded, why are they not impressed on the minds

of our law givers? Is it not an obvious truth, that human life is constantly a victim to the ancient and absurd mode of building cities, and that this evil, annually preying on our citizens, is infinitely more destructive than *imported* diseases? Yes, no doubt; the *crouded* manner of living in cities destroys *fifty lives*, to every *one* that falls prey to *imported* infection.

London requires 10,000 persons from the country annually to supply the waste of life, and keep its population good; that is, *one million in a century*. This number is not more than *half* of the actual destruction of lives, in consequence of her crouded population and its inseparable vices. By the improvements in the structure of the city, after the fire in 1666, that city has escaped the plague; but the old streets, which were not burnt, are still infested with a typhus fever, at all seasons of the year.

The cities in America are yet small; but large enough to exhibit their pernicious effects on health and life. And why should we pursue the Gothic mode of crouding men together, like bees in a hive? Most of the cities of the old world were erected as castles or places of defence, in time of war, when all Europe was scourged with the petty contests of feudal princes. They were calculated to accommodate as many people as possible. The consequence has been, that they have all, at times, been ravaged with plagues. It is a moderate estimate to suppose that one hundred millions of people have been carried off by the plague or other epidemic fevers, since the Christian era; and ninety nine of a hundred of all those victims have been *inhabitants of cities*; and their diseases the *sole* consequence of the mode of constructing them and of a wretched police.

If these things are so, why are legislators so attentive to shut out *infectious diseases from abroad*, while they take no method to prevent the generation of them *at home*? I must repeat the idea and urge it upon the guardians of public safety; the principle danger is *not* from abroad—that is an old monkish idea, that wisdom and experience ought to discard. The *principal source of danger is at home, it is in our cities; at our doors, and in our houses.*

We cannot *see* the poison that destroys us—it is invisible; but we can often perceive it in our nostrils. Can a man see the particles composing the effluvia from a rose or the woodbine? Not at all. Will not a small body of smoke from the snuff of a candle, no larger than a pea, dissipate and spread over a large room so as to be invisible, and yet extremely offensive? Is not the mephitic air of a well, which strikes instant death, invisible to the eye? So with the poisonous acids, extricated from every species of filth in hot weather —they are invisible—they are sometimes offensive to the smell—sometimes hardly perceivable, while they are insidiously seizing and undermining the vital organs.

The people of America have now an opportunity of providing against the terrible ravages of a disease, which frequently afflicts the old world. The plague is not sent miraculously to punish people for their sins—it is the

necessary consequence of causes which have been *created* or *augmented* by mankind themselves—Instead of being a miracle that cities are afflicted with the plague, it would be a miracle if they were to escape.

No, Sir; it is not possible, for any vigilance of the Police in cities built like Philadelphia, New York, Baltimore, Norfolk and Charleston, to prevent a frequent recurrence of that frightful disease. In New York, which is placed on a point of land, naturally as healthy as any in the United States, a situation as free from all *natural* causes, of disease, as it is possible to conceive one to be, the plague will rarely become epidemic. In *common* seasons, vigorous measures of washing and sweeping will limit the disease to a few cases of persons most exposed to the action of particular causes, or of peculiarly febrile habits. But when a powerful *general contagion* exists, and such a state of air certainly does return, at unequal periods, the low grounds even in New York will be ravaged with that disease in spite of all human efforts. Such is to be the penalty which nature will inflict on this city, for the extension of buildings beyond the limits which nature designed for the foundation of a city.

In other cities mentioned, the same disease will occur more frequently, and with greater violence. Governor Mifflin, in his address to the legislature of Pennsylvania, intimates that attention to cleanliness is sufficient to guard Philadelphia from domestic contagion. I am persuaded this idea is not correct. Cleanliness will do much—it will remove a fruitful source of contagion—it may, in favorable seasons, prevent an epidemic, and in all seasons, mitigate its violence. But I must maintain that our mode of building cities necessarily creates causes, adequate, in *bad* seasons, to the production of the plague, in spite of all efforts at cleanliness.

There are many cities in the South of Europe which are not often afflicted with the plague, yet at times they are laid desolate by that disease. But no part of the South of Europe, of any extent, is low marshy ground—it is most rough and hilly, and infinitely more healthy than the Atlantic coast of the middle and Southern States.

But admitting that no pestilential disease is ever generated in our cities, and that all the yellow fever that ever prevailed in the United States, has been imported, it makes no difference in the argument against the paltry Gothic manner of constructing towns. If the air of cities must be vitiated and the body disposed to the disease, before infection can be propagated; and if our cities do thus vitiate the air and prepare the medium by which infection *is* and *must* be communicated, the reason against building cities in the present form, exists in all its force; for it is found by experience that no human vigilance can prevent the introduction of sick or their cloathing. And if every accidental introduction of infection from the West Indies, with which these states have perpetual and increasing intercourse, dooms an immense city to the annual hazard of a terrible pestilence, and this, the advocates of *imported* origin will admit *must* be the case, even on their *own* principles, in

the name of humanity what shall be done? Sweeping, cleansing and washing are ineffectual, the laws of quarantine are found, in all countries, to be ineffectual, some lurking infection will, at times, escape the utmost vigilance of officers, and light up a flame to devour our citizens by thousands. What is to be done, sir, even on your own principles?

Believe me, sir, there is but one way, and that is for legislators to interpose and instantly stop the present mode of laying out and building towns for commerce and manufacturers. Those cities which are built must remain at present, the *sources of poison* and the *graves of men*. But provisions should be made by the state gradually to alter them and convert them into shady and healthful towns, by purchasing lots as fast as they became vacant, in sufficient numbers to render the streets open and airy.

There is no great difficulty in this business—it requires only wisdom and firmness, guided by zeal and knowledge.

Indeed the laying out and building a town, in the manner I have described, might be a most valuable object of speculation for individuals. Such a pleasant city, uniting the utility of the town with the salubrious air of the country, in the vicinity of New York or Philadelphia, would immediately command the residence of all the wealthiest citizens, and all the best business. Any company of men who could furnish a healthy capital to make a beginning, would find themselves amply reimbursed, with immense profit.

These observations are not intended solely for sea port towns. They are equally applicable to Albany, to Hartford in Connecticut, to Frederick town in Maryland; and all other growing towns on the margin of rivers or on extensive plains. All populous towns in the United States will hereafter be afflicted with malignant fevers and plague, unless a speedy and effectual stop should be put to 30 feet streets, 20 feet lots, and contiguous houses.

Thus sir, I have openly communicated to you and to my fellow citizens my reasons for differing from you in opinion, as to the origin of the pestilential disease which has proved formidable to many cities in our country. If, in these letters, any mistakes have occurred, which is probable, I shall be happy to see them detected and will readily abandon them. If, in any instance, I have intruded into the department which belongs exclusively to the professors of medicine, my excuse must be that I have endeavored to avoid it, as much as my subject would permit.—If any intemperate or uncandid language has escaped my pen, it shall when pointed out to me, be cheerfully expunged. These letters were begun without previous preparation —and have been prosecuted amidst other incessant employments.—They doubtless want correction and are certainly deficient in arrangement; but they have been written with a strong sense of the importance of the subject and an honest *zeal* to serve my country.

I am, Sir,

Your obedient servant,

N. WEBSTER, Jun.